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CONDUCTED BY

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AND

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. XXVII.—FIFTH SERIES.

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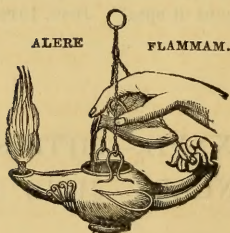
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“Meditationis est perscrutari occulta ; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phoebus ferrugine condāt,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



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[FIFTH SERIES.]

JANUARY 1889.

I. *Energy and Vision.* By S. P. LANGLEY*.

WHILE it is quite a familiar fact that the luminosity of any spectral ray increases proportionately to the heat in this ray, and indeed is but another manifestation of the same energy, I have recently had occasion to notice that there is, on the part of some physicists, a failure to recognize how totally different optical effects may be produced by one and the same amount of energy according to the wave-length in which this energy is exhibited.

I should not perhaps have thought it advisable to make this last remark, were it not that there has appeared in a recent number of Wiedemann's *Annalen* a paper by H. F. Weber on "The Emission of Light," in which he tacitly makes the assumption that the luminosity of a colour is proportionate to the energy which produces it, an assumption which it is surprising to find in a paper of such general merit and interest.

In another article of the same number of the journal the mistake was pointed out by Professor F. Stenger, who remarked that M. Weber's assumption was inconsistent with the

* From an advance proof communicated by the Author, to whom we are also indebted for the clichés.

Read in abstract before the National Academy of Sciences, April 19 1888.

investigations of the present writer. Still the fact that there could be such a misapprehension at the present day led me to look at the matter again, and to observe, with some surprise, that there was nowhere, in any physical work known to me, any exact or even approximately exact statement of the relative ocular effects of a given amount of energy in different parts of the spectrum. I have undertaken, therefore, during the last few months an experimental reinvestigation of this subject, with such a statement especially in view.

We shall evidently need two correlated sets of experiments; the first set to determine the amount of energy in each ray, the second to show the corresponding visual effect.

For the first of these, since energy only shows itself through absorptive media which more or less disguise it, we must select that manifestation which disguises it least; and in this respect beyond comparison the thermal one stands first, as the *heat* dispersed by a glass prism and shown by a thickly lampblackened thermometric apparatus is, throughout the visible spectrum, very nearly proportionate to the energy itself. For these first or thermal experiments, whence the energy is readily deduced, with close approximation, we shall rely principally upon a very elaborate investigation made here some time since and already published, where the bolometer is used to deduce in terms of lampblack absorption the relative amounts of solar energy in various wave-lengths throughout the visible spectrum and a little beyond; and which has been supplemented by a new investigation of the same kind in the present connexion.

Our second set of experiments will consist of a recent parallel series of photometric solar measurements taken at the same wave-lengths as the thermal ones, and which we may say gives this energy in terms of what I may perhaps be allowed to call, provisionally, "retinal" absorption.

The thickly lampblackened surface, then, and the retinal screen provided by nature in the eye, both exercise selective absorption; but the first, whose absolute absorption is here nearly total, does so in relatively so small a degree that we may, in the visible spectrum, provisionally neglect it, and consider the bolometric effect as here proportional to the energy itself.

It is evident that these two series once made, and reduced in both cases to the normal spectrum, would give us for any individual human eye the means of stating the visual effect in terms of absolute energy. The visual effect is known to vary in a very minute degree with the absolute amount of this

energy, at least if we admit the physiological influence of what has been called "the colour of brightness;" but for the comparatively feeble lights employed, this physiological effect seems to be almost negligible, and it is nearly immaterial within the limits of the experiment what unit of energy we take.

The object of these experiments, then, is to take some one constant amount of energy, to actually or virtually display it successively in different portions of the spectrum, and to observe in what proportion the optical or visual effects of this fixed amount of energy vary, according to the wave-length in which it is conveyed. While the measurements which insure this constancy are best made by thermal methods, and while the prism is, on the whole, far more convenient for them than the grating, it is nevertheless desirable to reduce the whole measurements to what they would have been if taken directly in the normal spectrum. The writer's measurements, already published and here cited later, afford the means of doing this with precision. These show that the energy is far from being distributed equally even in the normal spectrum; and that, according as it varies from one part of the spectrum to another, we must, by opening the aperture through which it is admitted where it is weak, and by narrowing it where the energy is strong, or by other like device, maintain it absolutely constant, or else (what is far better) let it enter through one fixed aperture, and use the subjoined table to apply a correction for the actual irregularities. Let it be remembered that we are now speaking of absolute energy, not of those physiological effects of it on the organ of vision which we call light; and it is to the value of this absolute energy for different wave-lengths in the normal spectrum which the subjoined table refers. This table, which gives the energy as derived from thermal experiments, rests on many thousand observations, taken, however, all with what is called a high sun, *i. e.* with a sun more than 30° above the horizon. As the distribution of this energy varies somewhat from day to day, and particularly in the violet and beyond, we have supplemented it by a series of direct observations taken with the bolometer on April 6, 1888, using the same glass prism employed in the photometric work described later. As those observations show a fair accordance with the others, it is not necessary to repeat them.

TABLE I.—Normal Spectrum.

$\lambda =$	$0\mu\cdot35$	$0\mu\cdot38$	$0\mu\cdot40$	$0\mu\cdot45$	$0\mu\cdot50$	$0\mu\cdot55$	$0\mu\cdot60$	$0\mu\cdot65$	$0\mu\cdot70$	$0\mu\cdot75$	$0\mu\cdot768$
Heat=	1·8	3·7	5·3	11·9	17·3	20·7	21·9	22·2	21·4	20·7	20·2

What has just been given in Table I. refers to the distribution of energy in terms of lampblack absorption, *i. e.* as “*heat*.” We now proceed to attempt to find it in terms of retinal absorption, *i. e.* as “*light*.” It is well-known that colour photometry offers peculiar difficulties. My own experience, after a long employment of the Rumford photometer for comparing the relative intensity of different coloured lights, is most unfavourable to it, and I have also tried the Bunsen photometer with almost equally unsatisfactory results. I have also experimented with the ingenious photometer described by Masson (*Ann. de Ch. et de Ph.* sér. 3, t. xiv. p. 129), in which a disk of paper marked with black and white sectors is revolved with such rapidity that it assumes a uniform tint when viewed by the coloured light in question, but when illuminated by the electric flash displays the sectors again. It is evident that the reappearance of the sectors under the flash will be conditioned by the nature of the light which furnishes the steady illumination. But though on trial this has seemed to yield better results than the ordinary photometers, the method is of difficult application in connexion with the particular apparatus about to be described. I have therefore, after considerable experiment, decided in favour of what may seem, at first, to be a cruder method, but which is, I believe, for the present purpose preferable to any of the foregoing; I mean the determination of the intensity of light necessary to read a table of logarithms or to discern any arbitrary characters.

Description of the Apparatus.

The measurements have all been made in a dark room from which every source of outside light is excluded except that which enters the slit of the spectroscop.

The light from the siderostat mirror M (fig. 1) passes through a small aperture in the north wall and falls on the slit (s_1) (which has doubly moving jaws, 34 millim. high, set in these experiments at a standard distance of 0·1 millim.), then on the great collimating-lens (l) of 755 centim. focus (aperture 11·9 centim.), t_1 being a paper tube to prevent the

lateral diffusion of light from dust particles. p is a glass prism*, m , the concave mirror of 148 centim. focus, which here forms upon a second slit (s_2) a spectrum about 7 millim. high and 90 millim. long in the easily visible part from A to H. The prism and mirror are mounted on the spectrometer already elsewhere described†, and which is provided with a circle reading to $10''$ of arc. By setting this circle, any colour can be brought on the slit (s_2). The light which the mirror has converged into that part of the spectrum overlying this slit passes through it, diverges and falls upon a black paper (fig. 2), in which is a central aperture 1 centim. square, occupied by part of a table of logarithms, printed in small black type on white paper. This table can be adjusted to bring different figures in view, but is otherwise fixed relatively to the black paper screen which (with this central square centimetre occupied by figures) is mounted on a slider. The rod (r) on which the slider moves is a prolongation of the spectroscope arm, made of a light wooden rod graduated so that one can read the position of the slider to a centimetre by *feeling* of notches in the dark. The zero of this rod is at slit 2 on which the spectrum is thrown.

It is to be observed that it is necessary that the square of figures should be small in order that it may be slid nearly to the apex of the cone of light and remain covered thereby.

It is to be noted also that at a constant distance and in a feeble light, these small figures may be invisible to the naked eye and most distinctly visible to the same eye with a magnifying glass. For two eyes of different foci, the amount of light with which the same figures will be read will probably vary. It follows that even if the same person read from beginning to end of the series, his readings will not be comparable unless they are all taken under the same optical conditions, *e. g.* all with the naked eye or all with glasses of a certain strength.

In these measurements a magnifying glass of 4.7 centim. focus was used by all the observers, and in addition, two who were near-sighted wore spectacles correcting this defect.

* Its principal constants are :—height of face 11.5 centim., width 10.5 centim., while for a temperature of 28°C . the refracting angle is $60^\circ 06' 45''$; deviation :

$$H = 46^\circ 45' 35''$$

$$b_1 = 44^\circ 45' 55''$$

$$D_2 = 44^\circ 11' 15''$$

$$A = 43^\circ 24' 05''$$

$$\omega_2 \text{ ("little Omega")} = 41^\circ 34'$$

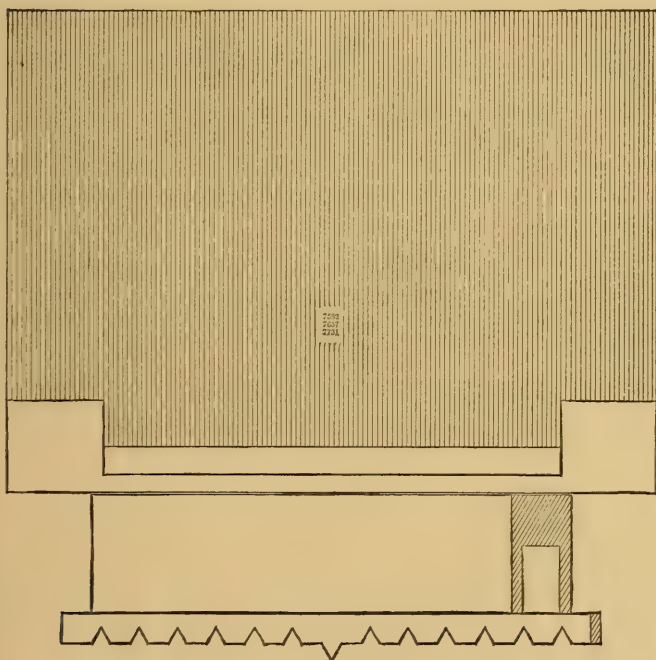
† "Researches on Solar Heat," Prof. Papers of the Sig. Serv., No. 15, p. 130.

The observer, in a room completely darkened, except for the minute light diffused from the particles in the reflected beam, and himself shielded even from the feeble light diffused from the surfaces of the lens, the prism, and the mirror, by the thick black curtain shown on the plan, waited until his eye had become quite sensitive before making the readings. An assistant outside the curtain set the circle by the aid of a dark lantern, and adjusted the siderostat from time to time so as to keep the light exactly on the centre of the lens and prism face. The passage of the slightest wisp of cirrus cloud was noted and the observer warned.

Although the light diverges from slit 2 and not from a point, the "cone of rays" above referred to, is, as regards the

Figure 2.

SCREEN, half full size.



object and limits of our experiments and the limiting positions of the screen, so nearly coincident with a geometrical cone, that, as the slider is carried away from the slit, the light may be treated as diminishing proportionally to the inverse square

of the distance from the slit to the screen. The nearest position of the screen brings it within 20 centim. of the slit, the furthest is over 300, so that we have the power of diminishing the light over $\left(\frac{300}{20}\right)^2$ or over 225 times. This, however, is by no means a sufficient range for the comparison of the light in the yellow-green with that in the extreme red; and because the graduated rod was not long enough to thus give the desired range, a photometer-wheel was introduced in some of the measurements between the siderostat mirror and the remote slit (s_1). This photometer-wheel is capable of reducing the light from .50 to .05 or further, and is more fully described in *Memoirs National Academy of Sciences*, vol. iii. *Memoir on the Temperature of the Moon*. We have, then, without altering the slit, a range of adjustment through over $\frac{225}{.05}$ or over 4500 times. The slit s_1 where the light first enters has doubly moving jaws, controlled by a micrometer-screw. Its standard opening in these experiments for light comprised between $\lambda = 0^{\mu}.40$ (violet) and $\lambda = 0^{\mu}.65$ (red) was 0.1 millim., but it has been opened for supplementary experiments to 5 millim., so that we have by opening or closing it a range of light from 50 to 1. It was, however, constantly kept at the standard opening of 0.1 millim. until the main series of experiments was completed, so as not to vary the light by attempting to reset it by the screw. Admitting, however, that for any given prism, transmitting any given ray, the light is sensibly proportional to the width of the slit (which may vary from 50 to 1), to the disposition of that coming through the photometer-wheel, which may vary from 20 to 1, and to the inverse square of the distance of the slider from slit s_2 (225 to 1), we have a possible range of $50 \times 20 \times 225 = 225,000$ to 1. This, however, it will be understood, has only been employed in our supplementary measurements.

In the following table all observations, whether made with or without the photometer-wheel, or with a wide slit, as in the case of the supplementary observations in the most feebly luminous portions at the extremities in the spectrum, have been reduced to these standard conditions :—

Photometer-wheel absent ;

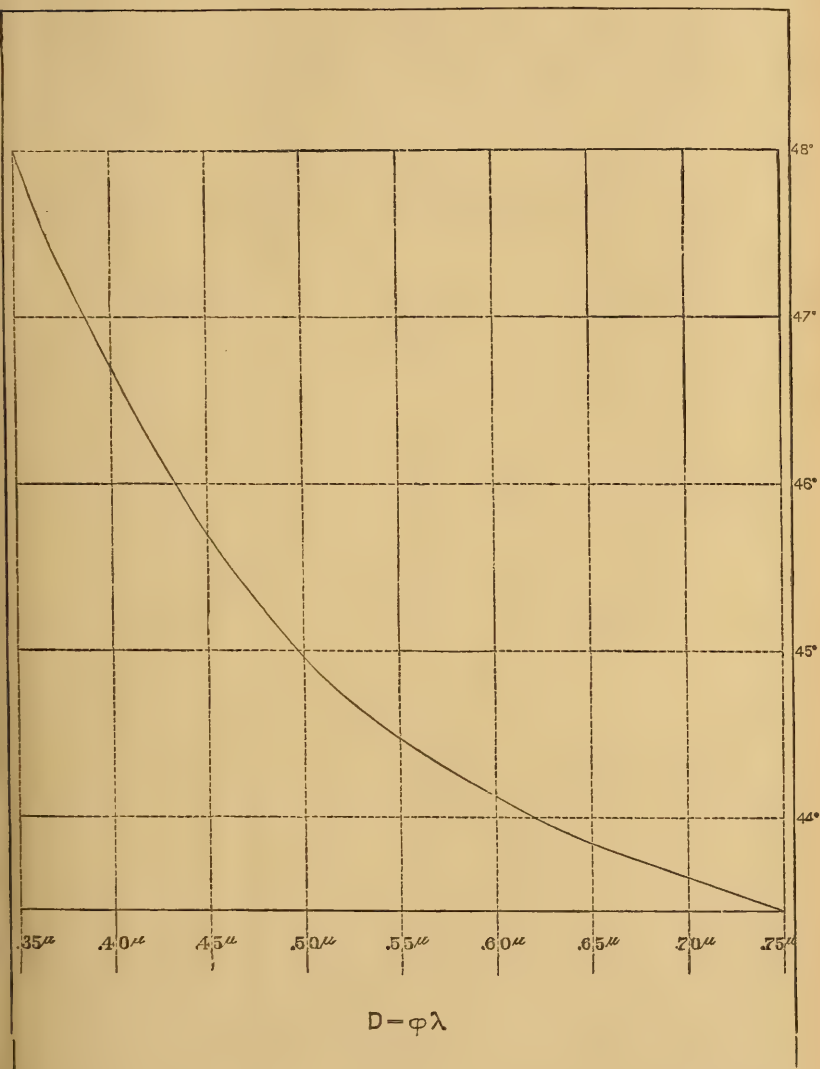
Slit (s_1) 0.1 millim. wide ;

Slit (s_2) 1 millim. wide ;

Slider with logarithm table at 1 metre from slit s_2 .

Each reading of the logarithms in the slider is taken when certain figures become discernible in the light in question,

Figure 3.



and is the mean of three independent observations, taken consecutively. In order to find the wave-length by means

of the prism we must prepare a table or a graphic construction, deduced from an examination of the special prism employed, showing the wave-length corresponding to the position of minimum deviation of each ray. Figure 3 is such a graphic construction derived from our own observations of the constants of the prism employed, and Table II. gives the approximate value of the tangents to the curve by means of which we pass from the prismatic to the normal scale.

TABLE II.

Approximate deviations and reducing factors (tangents) corresponding to adopted wave-lengths for great Hilger prism.

Wave-lengths. μ	Adopted. Tangents to curve.	Deviations. ° ' "
·35	2·28	48 00
·38	1·94	47 10
·40	1·73	46 42
·45	1·27	45 42
·50	·88	44 58
·55	·62	44 28
·60	·46	44 07
·65	·36	43 50
·70	·30	43 38
·75	·27	43 26
·768	·26	43 22

TABLE III.

Coefficients of Reflexions from two surfaces of Silver.

Wave-lengths.....	·35	·38	·40	·45	·50	·55	·60	·65	·70	·75
Percentage reflected from two surfaces	·37	·54	·63	·73	·79	·82	·845	·86	·875	·885
Reduction factor (reci- procal)	2·70	1·85	1·59	1·37	1·27	1·22	1·18	1·16	1·14	1·13

Table III. is a table for the selective absorption of silver referred to such a lamina as is spread by the Martin process on the front surface of the glass in its ordinary application. It is prepared from unpublished observations made by the writer with the bolometer in the course of the year 1881, and for the method of its preparation the reader

is referred to the footnote.* It will be seen from this table that while such a silver film exercises a considerable selective absorption in the ultra-violet, and even at the blue end, it exercises less as the wave-length increases, and in fact an extension of it would show a still enhanced power of reflexion for infra-red rays. It is with these infra-red rays that our measurements in previous researches on radiant heat at this Observatory have been hitherto mainly made. Accordingly our measurements of the selective reflexion in the ultra-violet, to which we have given comparatively little study, have not been repeated with all the care which the subject deserves, and we recommend a more complete determination of the selective absorption of silver there as an interesting field still open for experiment to those engaged in the study of that end of the spectrum.

By means of this graphic construction, which is amply accurate for the immediate purpose, and by the use of the formula already described (in *Memoirs of the National Academy*, vol. ii. p. 161), we can also pass from the actually observed prismatic spectrum to the effect which would have been observed in a truly normal one; and it is by the use of these constructions, founded on these formulæ, that the final reductions here given have been obtained. It is here assumed that no sensible selective absorption is exerted by the prism or any other portion of the apparatus.

We now give a summary of the photometric observations. The state of sky for each series and the approximate air-masses were :—

March 30th.—Sky “fair blue;” observer, S. P. L.; time, 11^h 40^m A.M. to 12^h 45^m P.M. (Greenwich 5th hour meridian time); air-mass†, 1.22 atmospheres.

April 2nd.—Sky “milky blue with cumuli;” observer, F. W. V.; time, 12^h to 2^h P.M.; air-mass, 1.19 atmospheres.

April 3rd.—Sky “blue with cumuli,” better than on 2nd, sky better in E. M.’s series than in that of F. W. V., when a slight haze, barely perceptible, had formed.

1st series: observer, E. M.; time, 11^h 10^m A.M. to 12^h 30^m P.M.; air-mass, 1.18 atmospheres.

* The selective absorption of silver has been deduced by bolometric measurements in the solar spectrum, with a Rutherford grating, by producing multiplied successive reflexions of the light from silver before allowing it to enter the slit of the spectroscope and determining successively the variation in the intensity of different rays according to the number of reflexions. The observations are reduced by a logarithmic formula.

† By air-mass is here meant that actually traversed by the solar rays, that with a vertical sun at sea-level being unity.

2nd series : observer, F. W. V. ; time, 1^h 15^m to 2^h 30^m P.M. ; air-mass, 1.28 atmospheres.

April 4th.—“A good blue at first, after 12^h milky blue from slight smoke, but still a fairly good sky.”

1st series : observer, F. W. V. ; time, 10^h 25^m to 11^h 55^m A.M. ; air mass, 1.23 atmospheres.

2nd series : observer, E. M. ; time, 12^h 30^m to 1^h 30^m P.M. ; air-mass, 1.20 atmospheres.

April 6th.—Sky “good blue, quite clear ;” observer, F. W. V. ; time, 10^h 45^m A.M. to 12^h 15^m P.M. ; air-mass, 1.18 atmospheres.

June 16th.—Sky “clear, good blue ; after 1 P.M. cirrus streaks ;” observer, B. E. L. ; time, 11^h 15^m A.M. to 1^h 45^m P.M. ; air-mass, 1.05 atmospheres.

July 2nd.—Sky “clear, excellent ;” observer, B. E. L.

1st series : time, 11^h 25^m A.M. to 12^h 15^m P.M. ; air-mass, 1.03 atmospheres.

2nd series : time, 12^h 15^m P.M. to 1^h 25^m P.M. ; air-mass, 1.02 atmospheres.

We first give in Table IV. the values of the photometric measurements in the prismatic spectrum, reduced to the standard conditions above cited.

TABLE IV.—Showing sensitiveness of the eye to light, as deduced from the power to decipher fine print. Prismatic (uncorrected) values :—

$\lambda =$	$0^{\mu} \cdot 35$	$0^{\mu} \cdot 38$	$0^{\mu} \cdot 40$	$0^{\mu} \cdot 45$	$0^{\mu} \cdot 50$	$0^{\mu} \cdot 55$	$0^{\mu} \cdot 60$	$0^{\mu} \cdot 65$	$0^{\mu} \cdot 70$	$0^{\mu} \cdot 75$	$0^{\mu} \cdot 79$
S. P. L. Mar. 30.				0.29	3.01	19.31	19.15	3.88	0.28		
F. W. V. April 2. April 3. April 4. April 6. Mean ...			0.13 0.30 0.20 0.17*	9.89 9.88 10.88	57.94 154.2 154.6	113.3 167.9 193.8	15.14 26.91 24.62	1.06 1.99 2.23 2.32	0.27 0.43 0.33		
	0.0015*	0.017*	0.17*	10.22	122.25	158.33	22.22	1.90	0.34	0.005*	0.00
	0.0015	0.017	0.20	10.22	122.25	158.33	22.22	1.90	0.34	0.005	0.00
B. E. L. June 16. July 2. July 2. Mean ...			0.24 0.23 0.34 0.27	30.60 10.89 35.38 25.62	157.2 125.7 186.1 156.3	217.0 142.3 158.7 172.7	36.98 33.96 70.16 47.03	3.39 4.54 5.98 4.64	0.17 0.73 0.75 0.55	0.001* 0.004*	
	0.000*	0.003*	0.23	10.89	125.7	142.3	33.96	4.54	0.73	0.004*	
	0.000	0.003	0.27	25.62	156.3	172.7	47.03	4.64	0.55	0.002	
E. M. April 3. April 4. Mean ...			0.35 0.19 0.27	30.60 8.34 19.47	100.1 46.39 73.25	146.1 75.27 110.69	49.82 42.05 45.94	5.85 3.04 4.45	1.46		

* Blue (cobalt) glass over slit s_1 .

In Table V. are the final values, corrected for loss of light by reflexion from silver surfaces and reduced to the normal spectrum.

TABLE V.
Photometric Values. Normal Spectrum.

	$0^{\mu}35$	$0^{\mu}38$	$0^{\mu}40$	$0^{\mu}45$	$0^{\mu}50$	$0^{\mu}55$	$0^{\mu}60$	$0^{\mu}65$	$0^{\mu}70$	$0^{\mu}75$	$0^{\mu}768$
S. L.				0.50	3.36	14.61	10.40	1.62	0.096		
F. W. V.											
April 2.			0.36	17.21	64.76	85.69	8.22	0.44	0.092		
April 3.			0.81	17.18	172.4	127.0	14.61	0.83	0.15		
April 4.			0.56	19.11	172.8	146.7	13.37	0.93	0.11		
April 6.	0.0092*	0.062*	0.47*					0.97			
Mean ...	0.0092	0.062	0.55	17.83	136.65	119.8	12.07	0.79	0.117	0.0015*	0.0003*
										0.0015	0.0003
B. E. L.											
June 16.			0.65	53.23	175.7	164.2	20.07	1.42	0.058	0.003*	
July 2.	0.000*	0.011*	0.63	18.95	140.5	107.6	18.44	1.90	0.25	0.012*	
July 2.			0.94	61.56	208.0	120.0	38.09	2.50	0.26		
Mean ...	0.000	0.011	0.74	44.58	174.7	130.6	25.53	1.94	0.189	0.008	
M.											
April 3.			0.95	53.23	111.9	110.5	27.05	2.44	0.50		
April 4.			0.52	14.51	51.84	56.94	22.82	1.27			
Mean ...			0.74	33.87	81.87	83.72	24.94	1.86	0.50		

* Blue (cobalt) glass over slit (s_1).

In this table we have, first, the wave-lengths corresponding to the observed angles of deviation, these values reaching from $0^{\mu}35$ in the ultra-violet to $0^{\mu}77$ near Fraunhofer's A on the extreme border of the visible red. It is to be observed, however, that the great mass of the observations which were taken without disturbing the slit reach from $0^{\mu}40$ in the deep violet to $0^{\mu}70$ in the deep red. The figures corresponding to $0^{\mu}35$, $0^{\mu}38$, $0^{\mu}75$, $0^{\mu}77$ are extremely difficult to obtain with precision and are given here as supplementary to the others. There are four observers:—

S. P. L., whose eye is somewhat long-sighted (making convenient the use of convex glasses of half-metre focus) and not sensitive to very feeble light; eyes otherwise believed to be in normal condition.

F. W. V., near-sighted, using glasses whose negative focus is 14 centim. The eye appears to be much less sensitive to

the red than to the violet. The retina of this eye is somewhat deficient in black pigment.

B. E. L., near-sighted, using glasses whose negative focus is 42 centim.

E. M., a boy of fifteen whose sight is perfect as far as known.

It will be remembered that throughout this table from $0^{\mu}40$ to $0^{\mu}70$ the light enters through a slit whose aperture is constant. If under these conditions the logarithm table can be just read when the slider is one metre from the second slit (s_2), the light would be represented by unity; if at two metres, by 4; if at three metres, by 9; and so on. As, however, we have already explained, the length of the rod being limited to but little over three metres, for the higher values we are obliged to introduce the photometer-wheel. For instance, the strongest light observed by F. W. V. was in the prismatic yellow-green corresponding to a wave-length of $0^{\mu}55$, where 193.8 was noted. Had the rod been really indefinitely prolongable, the slider would have needed to have been removed to the length of nearly 14 metres. To avoid this the photometer-wheel was interposed, reducing the light to $\frac{1}{20}$ and the actual distance of the slider from the slit (s_2)

was, as we may easily see, $\sqrt{\frac{193.8}{20}}$ or 3.11 metres. The

feeblest light which has been here measured with the standard slit is that by F. W. V. on April 2nd at wave-length $0^{\mu}40$, which is put down at .13, corresponding to a distance of 36 centim. from slit (s_2).

To make clear the way in which we pass from Table IV. to Table V., let us take any particular observation, for instance that already cited of April 4th by F. W. V. at $0^{\mu}55$ of 193.8. Referring either to the graphic construction, or to Table I., we find the value of the tangent (at $\lambda = 0^{\mu}55$) = 0.62 approximately, and $193.8 \times .62 = 120.16$. Our Table shows the reduction-factor for two surfaces of silver to be 1.22, whence the final reduced value becomes

$$1.22 \times 120.16 = 146.6.$$

And in this manner, from Tables II. and III. the remaining values in Table V. are derived from those in IV.; but here let it be observed that these values in Table V. do not yet represent what we wish, since they do not correspond in any exact sense to one constant amount of energy. It is true that they might at first sight appear to do so, since one

constant quantity of solar energy actually or virtually entered through the same constant width of the slit to produce them, and passed through one constant aperture at the second slit, and since, finally, the prismatic values are reduced to those in the normal spectrum; but, as the writer has shown, not only by theoretical deductions, from what is observed with the prism, but by very numerous measurements in the normal spectrum from a grating by means of a bolometer, the solar energy in the normal spectrum itself is very unequally distributed (see Table I.).

Since thermal and luminous effects vary proportionately in the same ray, it is to be observed that the values in Table I. furnish for each wave-length a divisor which gives not only the heat but the *brightness* which would have been observed had the prism dispersed the energy which fell on it in such a way that the same amount of energy fell in one part of the spectrum as in another, and thus we finally obtain the values in Table VI.

TABLE VI.—Sensitiveness of the Eye for a constant amount of Energy of varying Wave-length.

$\lambda =$	$0\mu.34$	$0\mu.38$	$0\mu.40$	$0\mu.45$	$0\mu.50$	$0\mu.55$	$0\mu.60$	$0\mu.65$	$0\mu.70$	$0\mu.75$	$0\mu.768$
S. P. L.				0.042	0.194	0.706	0.475	0.073	0.004		
F. W. V.	0.0051	0.0168	0.104	1.50	7.90	5.79	0.551	0.036	0.005	0.00007	0.00001
B. E. L.	0.000	0.0030	0.139	3.75	10.10	6.31	1.17	0.089	0.009	0.00004	
E. M. ...			0.140	2.85	4.73	4.04	1.14	0.084	0.023		
Mean*...	0.0026	0.0149	0.128	2.70	7.58	5.38	0.954	0.070	0.012	0.00006	0.00001

It will be observed that no correction has been introduced for selective absorption in the substance of the prism itself, as this is absolutely negligible within the limited range of the spectrum we are discussing.

This table exhibits the relative effect upon very different eyes of a given amount of energy in the form of radiation of various wave-lengths.

Quite notable differences exist between the different observers, not only as to the absolute sensitiveness of the eye, but also as to the relative efficiency for different colours. This seems to be, to some extent, a function of the age of the observer, if we may draw any conclusion from so few compa-

* The observations of S. P. L. are here omitted from the mean.

risons, the younger eyes being much more sensitive to the rays of shorter wave-length. Beyond this, any unusual efficiency for a particular part of the spectrum is, perhaps, apt to be balanced by a deficiency in another part, which, if strongly pronounced, would be termed colour-blindness. Prof. J. Clerk-Maxwell, employing pure spectrum-colours, formed white by combining 26·3 per cent. of red with 30·2 per cent. of green and 43·5 per cent. of blue (Phil. Trans. R. Soc. 1860, p. 79); and on another occasion, with a slightly different apparatus (*loc. cit.* p. 74), the same observer made white by mingling 21·9 per cent. of red with 33·3 per cent. of green and 44·8 per cent. of blue. The Allegheny observers F. W. V., B. E. L., and E. M., with whom this experiment was repeated, required from one fourth to one tenth less red and one sixth to one eighth more blue than Maxwell, forming white by mingling 20 per cent. of red with 30 per cent. of green and 50 per cent. of blue. Since, in order to make white, more of that colour is required for which the eye is most sensitive, we may, perhaps, infer that Prof. Maxwell was somewhat less sensitive to blue than these observers, although it should be remembered that the relative intensity of the blue and red in the solar spectrum is liable to undergo considerable fluctuations, so that where direct comparison of individual eyes is impossible, some uncertainty must remain.

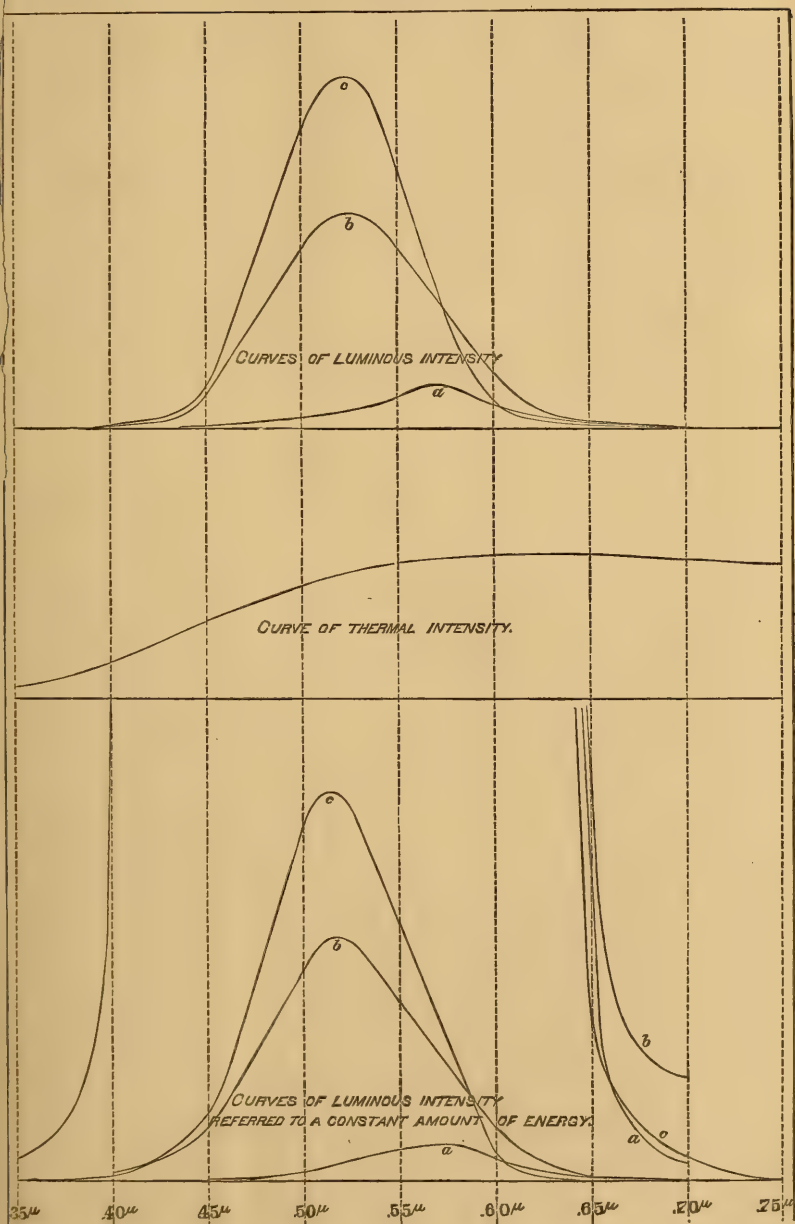
We have selected for comparison with our results the following by Capt. Abney (using a different photometric method), which we have here reduced to the normal scale. (See "Transmission of Sunlight through the Earth's Atmosphere," by Capt. W. de W. Abney, R.E., F.R.S., Phil. Trans. R. Soc. vol. clxxviii. (1887), A., pp. 274–276.) From the mean of the observations of July 1st, July 5th, and July 21st, 1886, made with an average air-mass of 1·33 atmospheres, we obtain these photometric values for the normal spectrum:—

$\lambda =$	0 μ ·40	0 μ ·45	0 μ ·50	0 μ ·55	0 μ ·60	0 μ ·65	0 μ ·70
Light =	0·8	2·8	25·0	82·0	66·5	12·3	0·5

The general form of this curve agrees with that of S. P. L. (curve *a*, fig. 4), showing a maximum sensitiveness near $\lambda = 0\mu\cdot57$. The light-curves of F. W. V. (curve *c*, fig. 4) and of E. M. (curve *b*, fig. 4) have their maxima respectively near $\lambda = 0\mu\cdot52$ and $\lambda = 0\mu\cdot53$.

Everything which has preceded has had reference to the *relative* luminous effects produced by *any* (moderate) constant quantity of energy. It may, however, be interesting to make the novel calculation as to the actual amount of energy either in horse-power or any other unit, required to make us *see*,

Figure 4.



and we can obtain an approximate estimate of this amount of energy as follows :—

Actinometric measurements made during the progress of the photometric observations showed a solar radiation of 1.5 calories per square centimetre per minute. Of this amount of heat the slit (s_1), being 3.4 cm. high by 0.01 cm. wide, received the fraction 0.034. The visible spectrum, from A to H included, according to the bolometer, measures about 21 per cent. of the total energy, the absorption of the lower infra-red by the great thickness of glass in the prism being large. We estimate that nearly 20 per cent. had been lost by reflexion before the bolometer was reached. The spectrum formed had a length of 86 mm. from A to H. The *average* energy which passed through the millimetre-aperture of slit s_2 was therefore (within these limits and expressed as heat)

$$1.5 \text{ cal.} \times 0.034 \times 0.21 \times 0.8 \times \frac{1}{86},$$

or approximately $\frac{1}{10000}$ calorie, let us say 4000 ergs per minute.

At 1 metre from slit s_2 this energy is further spread out over an illuminated area of 28 sq. cm., of which the square centimetre of fine print, being placed at an angle of 45° with the path of the ray, occupies only about $\frac{1}{40}$. If a length of one millim. of the standard spectrum receives an average energy of $\frac{1}{10000}$ calorie per minute, the actual working part of the screen, consisting of the little square of fine print, will receive at a distance of one metre $\frac{1}{400000}$ calorie per minute. But this by no means gives the amount of energy requisite to produce vision, since the eye is able to receive a distinct visual impression in less than one half second of time. We may say, therefore, that a luminous energy of $\frac{1}{50000000}$ calorie is sufficient to give a distinct view of the small square of figures in the brightest part of the spectrum, even after the immense loss of light by absorption and diffusion in the paper, which may amount to $\frac{19}{20}$ of the whole.

Even less light is needed to give the bare impression of luminosity. The sensitiveness of the human eye is indeed so extraordinary, that the chief difficulty in measuring its power is to find means for sufficiently reducing the intensity of sunlight which are at the same time capable of even approximate numerical estimation. Out of numerous plans tried, the following has given the most reliable result:—

In front of the first slit, in the path of the rays from the siderostat, was placed a plate of glass *very lightly* smoked,

whose transmission for different kinds of light was first photometrically measured and found to be

For violet light ($\lambda=0^{\mu}\cdot40$)	transmission	0.000210,
„ green „ ($\lambda=0^{\mu}\cdot55$)	„	0.000655,
„ red „ ($\lambda=0^{\mu}\cdot65$)	„	0.002350.

The photometer-wheel was next interposed, its aperture being sometimes reduced until only 2 per cent. of the light received passed through it.

The slit was at first kept as near the standard width of 0.1 mm. as possible ; but it was afterwards deemed best to secure the final adjustment for the *minimum visibile* at the slit, as it was evident on trial that the inaccuracy due to the varying loss by diffraction was small, compared with the inevitable uncertainty of the observer himself.

Finally, the larger part of the necessary reduction was secured by reducing the aperture of the collimating-lens by means of a metal plate pierced by a minute aperture whose area, 0.00015 sq. cm., was 0.000003 of the fully illuminated area of the lens.

The aperture of the human eye, according to Du Bois-Reymond's photograph (see *Nature*, May 3, 1888, p. 15), is about 0.7 sq. cm., when fully expanded, or the same as that of the foreshortened disk of figures previously employed. The size of the light spot at the standard distance beyond slit s_2 , when the minute aperture is placed over the collimating-lens, is reduced so that about two thirds of the light enters the eye placed 1 metre behind the 1 mm. slit on which the spectrum is formed.

The following reductions of sunlight were needed in order to give a light which approximated to the *minimum visibile*, defining this to be not the smallest light whose existence it is possible to suspect, or even to be reasonably certain of, but a light which is observed to vanish and reappear when silently occulted and restored by an assistant without the observer's knowledge.

Referred to the standard spectrum employed in the previous photometric work, the observer F. W. V. found :—

Fraction of standard* violet light ($\lambda=0^{\mu}\cdot40$) required for certain vision

$$=0.00021 \times 100 \times 0.000003 = 0.000000,063.$$

* By "standard" is here meant the light in 1 millim. of the standard spectrum, whose length from A to H was 86 millim.

Fraction of standard green light ($\lambda=0^{\mu}55$) required for certain vision

$$=0\cdot000655 \times 0\cdot033 \times 0\cdot000003 = 0\cdot000000,0000655.$$

Fraction of standard scarlet light ($\lambda=0^{\mu}65$) required for certain vision

$$=0\cdot00235 \times 2 \times 0\cdot000003 = 0\cdot000000,0141.$$

Fraction of standard crimson light ($\lambda=0^{\mu}75$) required for certain vision

$$=10 \times 0\cdot000003 = 0\cdot00003.$$

The measurements were made on July 3rd and 11th, the sky being a fairly good milky blue and the sun within one hour of the meridian.

Assuming that the energy per millimetre of the standard spectrum was $0\cdot000001$ calorie per half second for the wavelengths $0^{\mu}55$ and $0^{\mu}75$, we have from Table I:—

$$\text{For } \lambda=0^{\mu}40, \text{ energy} = 5\cdot3 \div (20\cdot7 \times 1,000,000) \text{ calorie}$$

$$,, \lambda=0^{\mu}65, ,, = 22\cdot2 \div (20\cdot7 \times 1,000,000) ,,$$

by means of which we reduce each of the above values to absolute measure, obtaining for the maximum value of the

Minimum Visible.

	μ	Reciprocal of	Reciprocal of
Violet ...	$0\cdot40$	63,000,000,000,000 cals.=	1,500,000 ergs.
Green ...	$0\cdot55$	15,000,000,000,000 ,,	= 360,000,000 ,,
Scarlet ...	$0\cdot65$	66,000,000,000,000 ,,	= 1,600,000 ,,
Crimson ..	$0\cdot75$	33,000,000,000,000 ,,	= 780 ,,

Stating these values in terms of horse-power we have

Minimum Visible.

	μ	h.p.
Violet	$0\cdot40$	$0\cdot000000,000000,00018000$
Green	$0\cdot55$	$0\cdot000000,000000,00000075$
Scarlet	$0\cdot65$	$0\cdot000000,000000,00017000$
Crimson (near A)	$0\cdot75$	$0\cdot000000,000000,34000000$

The measurement of the *minimum visible* is subject to variations of a much wider range than those of the photometric method, and may perhaps be in error by 100 per cent.*

* The relative sensitiveness of the eye of the observer in question (F. W. V.) for the extreme red or violet, as compared with its power of detecting green light, appears to be somewhat less when determined by the method of *minimum visible* than by the reading of fine print.

By the former we have:—

Sensitiveness, violet ..	($\mu\cdot40$) : green ($\mu\cdot55$) = 1 :	240.
,, scarlet ..	($\mu\cdot65$) : green ($\mu\cdot55$) = 1 :	230.
,, crimson ($\mu\cdot75$) :	green ($\mu\cdot55$) = 1 :	450,000.

The probable error of a series of ten readings of fine print, under the actual conditions of observation with a (feeble) standard luminosity, is determined for two of the observers as follows:—

	Violet Light. $\lambda=0^{\mu}.40$.	Orange-yellow light. $\lambda=0^{\mu}.60$.	Scarlet light. $\lambda=0^{\mu}.65$.
Probable Error of one observation.			
F. W. V.	Per cent. 5.53	Per cent. 1.76	Per cent. 3.14
E. M.	7.69	2.51	2.86
Probable Error of mean.			
F. W. V.	1.75	0.56	0.99
E. M.	2.44	0.80	0.90

The measurements with violet light were made June 19, 1888, "sky hazy blue, thin but uniform cirrus haze." Those at wave-lengths $0^{\mu}.60$ and $0^{\mu}.65$ were obtained on June 20, 1888, "sky hazy blue with cumuli, haze not so dense as on the 19th but possibly less uniform."

For a large part of the spectrum the probable error of a single reading does not exceed 4 per cent., but the error may considerably exceed this for the violet rays, *the eye requiring a much longer time to regain its sensitiveness for light of this colour than for any other*, so that for measurements in this region an hour's stay in the darkened room is none too much to develop the full power of an eye which has recently been exposed to the full sunshine.

Time required for Vision.

In connexion with the photometric measurements the time

Photometry by the reading of fine print gave for the same observer

Violet	sensitiveness of eye	=	0.104,000
Green	" "	=	5.790,000
Scarlet	" "	=	0.036,000
Crimson	" "	=	0.000,070

unity being the sensitiveness for yellow light: and the relative effect by this method is:—

Violet	($\mu.40$)	: green ($\mu.55$)	=	1 : 56.
Scarlet	($\mu.65$)	: green ($\mu.55$)	=	1 : 160.
Crimson	($\mu.75$)	: green ($\mu.55$)	=	1 : 83,000.

required for the perception of very faint-coloured lights was investigated. The method was an electrical one. There was automatic registration on a chronograph of the instant of exhibition, and determination of the instant of response as the observer pressed a key. The interval of course includes quite a train of distinct operations. According to Mendenhall (*American Journal of Science*, [3] vol. ii. p. 156), that portion of the action of brain nerve and muscle which produces the mechanical effect, and which may be called automatic, takes place in certainly but little over one tenth of a second. But the sensations which demand a conscious concentration of the attention, and especially those which require for their registration a decision of the judgment, occupy an interval several times as great. The perception of a light just at the verge of visibility probably involves an exercise of judgment—an answer to the question, “Do I see the light or do I not?”—although the question may not be consciously propounded, and accordingly this kind of perception may be included in that class of combined sensation and mental operation which involves a choice. Professor Mendenhall found for the time required to decide between red and white 0·443 sec., and to decide between a circle and a triangle 0·494 sec. We have found for the average of over 1000 observations of the disappearance or reappearance of a very faint light (perhaps 20 times as bright as the faintest perceptible), 0·507 sec., but corresponding measurements with a moderately bright spectrum, the light being about 10,000 times as intense as that called “very faint,” gave 0·242 sec., a number which is intermediate between the times found by Professor Mendenhall for the appearance of a white card (0·292 sec.) and that of an electric spark (0·203 sec.). We may therefore conclude that distinct vision for a very faint light demands about one half second of time, while the perception of light of ordinary brightness requires only about half that interval. It is possible that differences in the rapidity of the perception for lights of different colours might be detected on more exhaustive study, but none have been noted in these experiments other than those which were attributable to the variation of intensity.

It will be seen that quantitative measurements of the effect upon the eye of different rays whose luminosity varied in the proportion of 200,000 : 1, were actually obtained, and that it would have been possible to considerably exceed these limits, especially when it is considered that the photometric measurements were confined to lights of feeble intensity. Since it is possible to look directly at the sun for as short a time as one half

second, it is certain that the eye, by the combined adaptability of the iris and retina, can perceive lights whose intensities vary in the ratio of 1 to 1 000 000 000 000 000* (10)¹⁵.

It will be understood that the writer does not profess any competency in physiological optics, and that the preceding observations, and the conclusions reached from them, are both to be understood from the purely physical point of view. This being premised, we will summarize the paper in the following conclusions:—

The time required for the distinct perception of an excessively faint light is about one half second. A relatively very long time is, however, needed for the recovery of sensitiveness after exposure to a bright light, and the time demanded for this restoration of complete visual power appears to be greatest when the light to be perceived is of a violet colour.

The visual effect produced by any given constant amount of energy varies enormously according to the colour of the light in question. It varies considerably between eyes which may ordinarily be called normal ones, but an average gives the following proportionate results for seven points in the normal spectrum, whose wave-lengths correspond approximately with those of the ordinary colour divisions, where unity is the amount of energy (about $\frac{1}{1000}$ erg) required to make us see light in the crimson of the spectrum near A, and where the preceding wave-lengths given correspond approximately to the six colours violet, blue, green, yellow, orange, red:—

Colour.	Violet.	Blue.	Green.	Yellow.	Orange.	Red.	Crim.
Wave-length..	μ.40	μ.47	μ.53	μ.58	μ.60	μ.65	μ.75
Luminosity ..	1,600	62,000	100,000	28,000	14,000	1200	1
(Visual effect.)							

Since we can recognize colour still deeper than this crimson, it appears from this that the same amount of energy may produce *at least* 100,000 times the visual effect in one colour of the spectrum that it does in another, and that the *vis viva* of the waves whose length is 0μ.75, arrested by the ordinary retina, represents work done in giving rise to the sensation of crimson light of 0.0000000000003 horse-power, or about 0.001 of an erg, while the sensation of green can be produced by 0.000000,01 of an erg.

* It may be interesting to check this result by an entirely different method. The light of the sun is, according to Pickering, equal to that of a star of -25.5 stellar magnitude, or 4400,000 000 times that of Sirius (mag. -1.4) which again is about 910 times that of a sixth magnitude star, ordinarily considered the faintest visible to the naked eye. Here the light of the sun is to that of the *minimum visibile* as 1 to 4,000 000,000 000 (4×10^{12}), but the difference seems accounted for by the fact that the ratio by this latter method is found for an eye exposed in starlight, by the former for an eye in *absolute* darkness.

II. *A Design for a Standard of Electrical Resistance.* By J. A. FLEMING, M.A., D.Sc., Professor of Electrical Technology in University College, London*.

[Plate I.]

IN designing a standard of electrical resistance the two points to which attention is directed are the choice of the material in which the standard is embodied, and the form or disposition of the instrument.

Experience is yet far from complete as to the entire permanence of wires of alloys over prolonged periods of time when employed as standards of electrical resistance ; but having regard to the inconveniences which attend the use of mercury in standards intended to be conveyed about, evidence, as far as we have it, points to the tolerable permanence of the platinum-silver alloy (66 p.c. of silver + 32 p.c. of platinum) when drawn into wire, for use as the material substance of which the actual standard is made.

A definite length and gauge of standard wire has then to be so arranged that, whilst kept at a constant temperature, currents can be passed through it and the resistance between certain points ascertained.

The form which has hitherto been chiefly manufactured, and which is in most general use, is the form of standard which was designed by the Committee of the British Association on the original introduction of the B.A. unit, and shown in plate 4 of the 'Reprint of Reports on Electrical Standards,' by Prof. Fleeming Jenkin. In this form of standard the actual coil is wound on a bobbin consisting of a tube of thin brass having ebonite cheeks. Attached to these cheeks are the two long bent copper rods which serve as the electrodes, held in position by a distance-piece of ebonite. In order that the coil may be immersed in a medium of known temperature it is further enclosed in a thin shell of brass consisting of a double tube (see fig. 1), and the whole shell filled up with paraffin wax or ozokerit. Some makers then place a thin lid of ebonite on the top of the shell.

Experience gained by a rather extensive use of standards of resistance of this form has indicated to the writer that this edsign can be, with some advantage, modified. The disadvantages of the present B.A. form of standard are as follows:—When in use the standards must be placed in water of a known temperature or in melting snow or ice. After a sufficiently prolonged time the temperature of this water can be taken, and the temperature of the water will be the temperature of

* Communicated by the Physical Society : read November 10, 1888.

the wire of the standard, assuming that equilibrium of temperature has been attained. If a current is now passed through the coil in order to take a measurement of its electrical resistance, the temperature of the wire is raised, and its resistance is altered.

Other things being equal, the best design of coil is that in which this electrically developed heat is got rid of by diffusion as quickly as possible. The embedding of a coil in a large mass of badly-conducting material like paraffin or ozokerit is, from this point of view, a great disadvantage.

Sufficient electrical insulation has to be provided ; but this should be achieved without the use of more enveloping insulation than necessary.

The two chief objections to the B.A. form of standard are, however, these :—

First, it cannot be placed in water with the shell wholly under water or under ice without short-circuiting the electrodes, and, when used as intended, whilst the narrow or bottom portion of the coil is in the water, the upper and more massive portion is in the air, and therefore may be at a different temperature to the bottom portion. Hence arises a doubt as to the actual temperature of the coil of wire. It has to be borne in mind that the limitation of accuracy in such comparisons of standards of resistance is determined by the difficulty of ascertaining temperature, and not in the mere measurement of resistance. Uncertainty as to the actual temperature of the wire to the extent of one or two tenths of a degree Centigrade renders nugatory elaborate arrangements for very accurate measurement of resistance.

Second. The standards, as at present constructed, are liable to another defect. If the standard is being used in melting ice or snow, and therefore cooled to 0° Cent., deposition of dew will take place upon the upper surface, whether the ebonite lid or paraffin-wax surface, through which the copper-rod electrodes protrude. The copper rods are originally lacquered or varnished, but when the lacquer wears off, any film of moisture so deposited will short-circuit the electrodes and reduce the observed resistance. In comparing standards in melting ice, either then the whole shell must be as far as possible placed under the melting ice, in which case stirring the liquid may splash water on to the surface of the paraffin, or else the shell has to be only partly immersed, in which case ambiguity exists as to the actual temperature of the coil of wire.

These and some other difficulties, such as that of keeping a rather deep vessel of melting ice at a constant temperature, have impressed on the writer the necessity for modifying the

form of the standard, and one form which has proved itself to be very satisfactory in use is as follows:—The case or shell which contains the coil is in the form of a ring (see fig. 5). This ring consists of a pair of square-sectioned circular troughs provided with flanges which can be screwed together so as to form a square-sectioned, hollow, circular ring.

From this ring proceed upwards two brass tubes about five or six inches in length. Down these brass tubes pass the copper electrodes or rods, and these rods are insulated from the tubes at the top and bottom by ebonite insulators. The insulator at the bottom of the tube, where it enters the ring, is a simple collar, that at the top has the form of a funnel corrugated on its outer surface. The use of this funnel will be referred to presently. The actual resistance-coil is a length of platinum-silver wire three-fold silk-covered. The silk-covered wire is first baked above 100° C. to dry it completely, and then immersed in melted ozokerit or paraffin.

The so insulated wire is cut about the proper length and laid double or folded once upon itself and then rolled up on a wooden mandril so as to form a circular coil of diameter suitable to drop into the hollow of the brass ring. The wire being wound double, its coefficient of self-induction is rendered very small. This coil of wire is then wrapped over with white silk and again dipped in melted ozokerit. The ends of the wire are next soldered into nicks in the ends of the copper rods, they having been previously pushed a little way through the brass tubes for the purpose, and afterwards drawn back into proper positions. The coil is then packed into the circular groove, and, after adjusting the resistance to the proper value, the bottom half of the ring is placed over it. A thin washer of indiarubber is inserted between the flanges, and the whole screwed tightly together. The resistance-coil is thus enclosed in a thin ring of metal, and can be placed wholly below the surface of water or ice. In order to test the tightness of the joints, a little test-pipe is provided on the upper surface of the ring. By placing the ring coil below water and blowing into the test-pipe, the good fitting of the joints can be assured. The aperture of this test-pipe is afterwards closed by solder or a screw (see fig. 6).

Apart from the insulation of the coil itself it will be apparent that the insulation is limited by the amount of insulation resistance secured at the ebonite insulators at the top end of the brass tubes. Any leakage from the copper rod over these insulators to the brass tube destroys to that extent the insulation of the coil. The object of making these external insulators funnel-shaped is to prevent surface creeping, due to condensation of moisture on them, by placing paraffin oil or insulating liquid in the funnel-shaped cavity. When this

is done, even if dew should collect on the outer surface of the funnels, the inner surface is kept dry by the paraffin oil placed in them, the action being the same as that in the well-known Johnson and Phillip's fluid insulator.

The ring-coils when in use are placed in rather shallow zinc troughs, which can be filled with water, and which are closed with a wooden lid. When so placed the whole of the actual coil or resistance part is down beneath the liquid at one level, where the temperature can be accurately ascertained. The insulators and point of emergence of the electrodes are away up above the level of the water, and well protected from any action which might permit of leakage over them. The large metallic mass of the ring assists in bringing the resistance-coil quickly back to the temperature of the surrounding water, and the coil therefore "tests quickly." In all other respects these standards of resistance are as compact and portable, and not more expensive to construct than the old form of B. A. standard, whilst obviating the difficulties which present themselves in the use of the old form in very accurate comparisons of resistance.

Other forms of standard coil which have been tried are indicated in figs. 2, 3, and 4, but have not proved themselves to be as convenient as that above described and illustrated in figs. 5 and 6.

It is quite possible to have two or more coils of wire inside the same ring, each coil having its separate pair of electrodes. A useful coil of this form can be made up containing 1, 10, and 100 ohms, so that comparisons can be quickly made at the same temperature with these three multiples of the same unit of resistance.

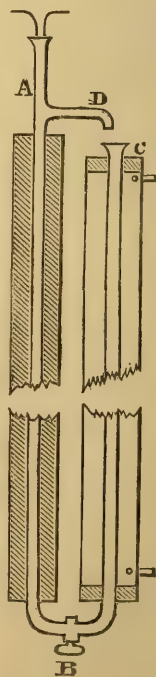
The adjustment of the coils to a certain value presents no great difficulties. The wire is in the first instance cut a little longer than required, and its resistance nearly adjusted; when the two ends of the coil have been soldered to the lower ends of the copper rods, the resistance is again taken from the ends of the electrodes. This resistance should be a little greater than the final value required. The middle point of the wire or extreme loop is now stripped of its silk and the loop twisted up with the pliers, the resistance being carefully taken at intervals. When just a very little in excess of the value required the twisted coil is touched with solder, and having been bound over with insulating material the coil is completed. In the construction of standards it is obvious that it is not so important that the resistance should have an exact integer value at any temperature as that its value at some temperature and its coefficient of variation of temperature should be exactly known.

III. *A Continuous Heat and Electrical-Current Measuring-Instrument.* By FREDERICK J. SMITH, M.A., *Millard Lecturer, Trinity College, Oxford, and Lecturer in the University in Experimental Mechanics and Physics**.

WHEN a quantity of heat given to a known mass of liquid causes it to rise through a certain number of degrees of temperature, this quantity of heat can be determined ; but if heat be continuously applied, then the liquid would reach a temperature quite inconvenient for experimental purposes.

By means of the following device, which the author thinks might be perhaps of interest to those who may be working in the same direction, heat may be imparted to a mass of liquid, while the quantity of heated liquid may be measured, cooled, and reheated continuously. In the figure is shown a section through the middle of the instrument.

A B C is a U-shaped tube furnished with a branch at D. The leg, C, is surrounded with a large tube, through which water at any required temperature may flow when required. The leg, A, is surrounded with cotton-wool or any good nonconductor of heat ; at B there is a stopcock. Either two parallel wires or a coil of wire extend through nearly the whole length of the leg A. The instrument is used thus :— The stopcock B is opened, and the U-tube is filled with mineral oil up to about 5 millim. from the top of C. An electrical current sent through the coil or wires causes the liquid in A to be heated, and therefore to increase in length. When it reaches the branch D it runs over and drops into the leg C, which is always at a lower level than D, owing to its being at a lower temperature, and consequently having a greater density than the liquid in the leg A. The number of drops in a given time, under certain circumstances, becomes an index of the heat given to the liquid in A, and therefore of the current by which the heat is produced.



* Communicated by the Author.

The author finds that, by making the leg A about 2 metres long, abundant length can be obtained between D and C to place a small fluid meter, by which means the weight of liquid which passes from D to C can be accurately measured. The instrument works either with a continuous or alternating current. The stopcock B is used to contract the cross section of the U-tube ; without some check a violent oscillation is set up between the two columns of liquid.

The first experimental instrument constructed by the author consisted of a continuous rectangular-shaped tube, one part of which was heated while the rest of the tube was kept cool. The liquid circulated, and its rate of circulation, when properly interpreted, would have been a measure of what was required ; but it was given up, as it appeared nearly impossible to cause the current of liquid to mechanically record its rate of flow with any amount of accuracy.

Several rather interesting results, which at present are not complete for publication, have cropt out of experiments made with the instrument. Of these, perhaps, the most promising was an illustration of the conversion of work into heat. The meter devised for determining the flow is one by which the weight of the oil is indicated, not the volume.

IV. *The General Solution of Maxwell's Electromagnetic Equations in a Homogeneous Isotropic Medium, especially in regard to the Derivation of special Solutions, and the Formulæ for Plane Waves.* By OLIVER HEAVISIDE*.

1. **EQUATIONS of the Field.**—Although, from the difficulty of applying them to practical problems, general solutions frequently possess little practical value, yet they may be of sufficient importance to render their investigation desirable, and their applications examined as far as may be practicable. The first question here to be answered is this. Given the state of the whole electromagnetic field at a certain moment, in a homogeneous isotropic conducting dielectric medium, to deduce the state at any later time, arising from the initial state alone, without impressed forces.

The equations of the field are, if p stand for d/dt ,

$$\text{curl } \mathbf{H} = (4\pi k + cp) \mathbf{E}, \quad . \quad . \quad . \quad (1)$$

$$-\text{curl } \mathbf{E} = (4\pi g + \mu p) \mathbf{H}; \quad . \quad . \quad . \quad (2)$$

the first being Maxwell's well-known equation defining electric

* Communicated by the Author.

current in terms of the magnetic force \mathbf{H} , k being the electric conductivity and $c/4\pi$ the electric permittivity (or permittance of a unit-cube condenser), and \mathbf{E} the electric force; whilst the second is the equation introduced by me* as the proper companion to the former to make a complete system suitable for practical working, g being the magnetic conductivity and μ the magnetic inductivity. This second equation takes the place of the two equations

$$\left. \begin{aligned} \mathbf{E} &= -\dot{\mathbf{A}} - \nabla \Psi, \\ \text{curl } \mathbf{A} &= \mathbf{H}, \end{aligned} \right\} \dots \dots \dots (3)$$

of Maxwell, where \mathbf{A} is the electromagnetic momentum at a point and Ψ the scalar electric potential. Thus Ψ and \mathbf{A} are murdered, so to speak, with a great gain in definiteness and conciseness. As regards g , however, standing for a physically non-existent quality, such that the medium cannot support magnetic force without a dissipation of energy at the rate $g\mathbf{H}^2$ per unit volume, it is only retained for the sake of mathematical completeness, and on account of the singular telegraphic application in which electric conductivity is made to perform the functions of both the real k and the unreal g .

Let

$$\left. \begin{aligned} \rho_1 &= 4\pi k/2c, & \rho &= \rho_1 + \rho_2, & v &= (\mu c)^{-\frac{1}{2}}, \\ \rho_2 &= 4\pi g/2\mu, & \sigma &= \rho_1 - \rho_2, \end{aligned} \right\} \dots \dots (4)$$

The speed of propagation of all disturbances is v , and the attenuating effects due to the two conductivities depend upon ρ_1 and ρ_2 , whilst σ determines the distortion due to conductivity.

2. *General Solutions.*—Let q^2 denote the operator

$$q^2 = -(v \text{ curl})^2 + \sigma^2; \dots \dots \dots (5)$$

or, in full, when operating upon \mathbf{E} for example,

$$q^2 \mathbf{E} = v^2 \nabla^2 \mathbf{E} - v^2 \nabla \text{div } \mathbf{E} + \sigma^2 \mathbf{E}. \dots \dots (6)$$

Now it may be easily found by ordinary "symbolical" work which it is not necessary to give, that, given \mathbf{E}_0 , \mathbf{H}_0 , the values of \mathbf{E} and \mathbf{H} when $t=0$, and satisfying (1) and (2), those at time t later are given by

$$\left. \begin{aligned} \mathbf{E} &= \epsilon^{-\rho t} \left[(\cosh qt - \frac{\sigma}{q} \sinh qt) \mathbf{E}_0 + \frac{\sinh qt}{q} \cdot \frac{\text{curl } \mathbf{H}_0}{c} \right], \\ \mathbf{H} &= \epsilon^{-\rho t} \left[(\cosh qt + \frac{\sigma}{q} \sinh qt) \mathbf{H}_0 - \frac{\sinh qt}{q} \cdot \frac{\text{curl } \mathbf{E}_0}{\mu} \right]. \end{aligned} \right\} (7)$$

* "Electromagnetic Induction and its Propagation," the 'Electrician,' January 3, 1885, and later.

A sufficient proof is the satisfaction of the equations (1), (2), and of the two initial conditions.

An alternative form of (7) is

$$\left. \begin{aligned} \mathbf{E} &= \epsilon^{-\rho t} \left[\cosh qt + \frac{\sinh qt}{q} (p + \rho) \right] \mathbf{E}_0, \\ \mathbf{H} &= \epsilon^{-\rho t} \left[\cosh qt + \frac{\sinh qt}{q} (p + \rho) \right] \mathbf{H}_0, \end{aligned} \right\} \quad \cdot \cdot \quad (7a)$$

showing the derivation of \mathbf{E} from \mathbf{E}_0 and $p\mathbf{E}_0$ in precisely the same way as \mathbf{H} from \mathbf{H}_0 and $p\mathbf{H}_0$. In this form of solution the initial values of $p\mathbf{E}_0$ and $p\mathbf{H}_0$ occur. But they are not arbitrary, being connected by equations (1), (2). The form (7) is much more convenient, involving only \mathbf{E}_0 and \mathbf{H}_0 as functions of position, although (7a) looks simpler. The form (7) is also the more useful for interpretations and derivations.

If, then, \mathbf{E}_0 and \mathbf{H}_0 be given as continuous functions admitting of the performance of the differentiations involved in the functions of q^2 , (7) will give the required solutions. The original field should therefore be a real one, not involving discontinuities. We shall now consider special cases.

3. *Persistence or Subsidence of Polar Fields.*—We see immediately by (7) that the \mathbf{E} resulting from \mathbf{H}_0 depends solely upon its curl, or on the initial electric current, and, similarly, that the \mathbf{H} due to \mathbf{E}_0 depends solely upon its curl, or on the magnetic current. Notice also that the displacement due to \mathbf{H}_0 is related to \mathbf{H}_0 in the same way as the induction $\div -4\pi$ due to \mathbf{E}_0 is related to \mathbf{E}_0 . Or, if it be the electric and magnetic currents that are considered, the displacement due to electric current is related to it in the same way as the induction $\div 4\pi$ due to magnetic current is related to it.

Observe also that in passing from the \mathbf{E} due to \mathbf{E}_0 to the \mathbf{H} due to \mathbf{H}_0 the sign of σ is changed.

By (7) a distribution of \mathbf{H}_0 which has no curl, or a polar magnetic field, does not, in subsiding, generate electric force; and, similarly, a polar electric field does not, in subsiding, generate magnetic force. Let then \mathbf{E}_0 and \mathbf{H}_0 be polar fields, in the first place. Then, by (5),

$$q^2 = \sigma^2,$$

that is, a constant, and using this in (7) we reduce the general solutions to

$$\mathbf{E} = \mathbf{E}_0 \epsilon^{-2\rho_1 t}, \quad \mathbf{H} = \mathbf{H}_0 \epsilon^{-2\rho_2 t} \quad \cdot \cdot \cdot \quad (8)$$

The subsidence of the electric field requires electric conductivity, that of the magnetic field requires magnetic conductivity;

but the two phenomena are wholly independent. The first of (8) is equivalent to Maxwell's solution*. The second is its magnetic analogue.

As, in the first case, there must be initial electrification, so in the second, there should be "magnetification," its volume-density to be measured by the divergence of the induction $\div 4\pi$. Now the induction can have no divergence. But it might have, if g existed.

There is no true electric current during the subsidence of \mathbf{E}_0 , and there would be no true magnetic current during the subsidence of \mathbf{H}_0 . In both cases the energy is frictionally dissipated on the spot, or there is no transfer of energy†. The application of (8) will be extended later.

4. *Purely Solenoidal Fields*.—By a purely solenoidal field I mean one which has no divergence anywhere. Any field vanishing at infinity may be uniquely divided into two fields, one of which is polar, the other solenoidal; the proof thereof resting upon Sir W. Thomson's well-known theorem of Determinancy. Now we know exactly what happens to the polar fields. Therefore dismiss them, and let \mathbf{E}_0 and \mathbf{H}_0 be solenoidal. Then

$$q^2 = v^2 \nabla^2 + \sigma^2, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where ∇^2 is the usual Laplacean operator. Of course $\cosh qt$ and $q^{-1} \sinh qt$ are rational functions of q^2 , so that if the differentiations are possible we shall obtain the solutions out of (7).

5. *Non-distortional Cases*.—Let the subsidence-rates of the polar electric and magnetic fields be equal. We then have

$$\left. \begin{aligned} \sigma &= 0, & q^2 &= -(v \operatorname{curl})^2, \\ & & \rho &= 4\pi k/c = 4\pi g/\mu, \end{aligned} \right\} \quad . \quad . \quad . \quad (10)$$

in the solutions (7). The fields change in precisely the same manner as if the medium were nonconducting, as regards the relative values at different places; that is, there is no distortion due to the conductivities; but there is a uniform subsidence all over brought in by them‡, expressed by the factor $e^{-\rho t}$. This property I have explained by showing the opposite nature of the tails left behind by a travelling plane wave according as σ is + or -.

* Vol. i. chap. x. art. 325, equation (4).

† This is of course obvious without any reference to Poynting's formula. The only other simple case of no transfer of energy which had been noticed before that formula is that of conduction-current, kept up by impressed force so distributed as to require no polar force to supplement it.

‡ "Electromagnetic Waves," Part I. § 7, Phil. Mag. February 1888.

The above applies to a homogeneous medium. But, if in

$$\text{curl } (\mathbf{H} - \mathbf{h}) = (4\pi k + cp)\mathbf{E}, \quad \} \quad . \quad . \quad . \quad (1a)$$

$$\text{curl } (\mathbf{e} - \mathbf{E}) = (4\pi g + \mu p)\mathbf{H}, \quad \} \quad . \quad . \quad . \quad (2a)$$

differing from (1), (2) only in the introduction of impressed forces \mathbf{e} and \mathbf{h} , we write

$$(\mathbf{H}, \mathbf{h}, \mathbf{E}, \mathbf{e}) = (\mathbf{H}_1, \mathbf{h}_1, \mathbf{E}_1, \mathbf{e}_1)\epsilon^{-\rho t},$$

we reduce them to

$$\left. \begin{aligned} \text{curl } (\mathbf{H}_1 - \mathbf{h}_1) &= c(\sigma + p)\mathbf{E}_1 \\ \text{curl } (\mathbf{e}_1 - \mathbf{E}_1) &= \mu(-\sigma + p)\mathbf{H}_1 \end{aligned} \right\} \quad . \quad . \quad . \quad (11)$$

and these, if $\sigma = 0$, are the equations of a nonconducting dielectric. That is,

$$\rho = 4\pi k/c = 4\pi g/\mu = \text{constant}$$

is the required condition. Therefore c and μ may vary anyhow, independently, provided k and g vary similarly*. The impressed forces should subside according to $\epsilon^{-\rho t}$, in order to preserve similarity to the phenomena in a nonconducting dielectric.

Observe that there will be tailing now, on account of the variability of $(\mu/c)^{\frac{1}{2}}$ or μv . That is, there are reflexions and refractions due to change of medium. The peculiarity is that they are of the same nature with as without conductivity.

6. *First Special Case.*—A special case of (11) is given by taking $\mu = 0$ and $g = 0$; that is, a real conducting dielectric possessing no magnetic inductivity, in which k/c is constant. If the initial field be polar, then

$$\mathbf{E} = \mathbf{E}_0\epsilon^{-\rho t}, \quad \mathbf{H} = 0. \quad . \quad . \quad . \quad (12)$$

This extension of Maxwell's before-mentioned solution I have given before, and also the extension to any initial field, and the inclusion of impressed forces†. The theory of the result has considerable light now thrown upon it.

If the initial field be arbitrary, the solenoidal part of the flux displacement disappears instantly, therefore (12) is the solution, provided \mathbf{E}_0 means the polar part of the initial field; that is, \mathbf{E}_0 must have no curl, and the flux $c\mathbf{E}_0/4\pi$ must have the same divergence as the arbitrarily given displacement.

Now an impressed force \mathbf{e} produces a solenoidal flux only. Therefore it produces its full effect and sets up the appropriate

* In § 4 of the article referred to in the last footnote the property was described only in reference to a homogeneous medium.

† "Electromagnetic Induction," 'Electrician,' December 18, 1885, and January 1, 1886.

steady flux instantaneously ; and all variations of \mathbf{e} in time and in space are kept time to without lag by the conduction-current in spite of the electric displacement.

This property is seemingly completely at variance with ideas founded upon the retardation usually associated with combinations of resistances and condensers. But, being a special case of the nondistortional theory, we can now understand it. For suppose we start with a nonconducting dielectric, and put on \mathbf{e} uniform within a spherical portion thereof, and send out an electromagnetic wave to infinity and set up the steady flux. On now removing \mathbf{e} , we send out another wave to infinity, and the flux vanishes. Now make the medium conducting, with both conductivities balanced, as in (10). Starting with the same steady flux, its vanishing will take place in the same manner precisely, but with an attenuation factor $\epsilon^{-\rho t}$. Now gradually reduce g and μ at the same time, in the same ratio. The vanishing of the flux will take place faster and faster, and in the limit, when both μ and g are zero, will take place instantly, not by subsidence, but by instantaneous transference to an infinite distance when the impressed force is removed, owing to v being made infinite.

7. *Second Special Case.*—There is clearly a similar property when $k=0$ and $c=0$, that is, in a medium possessing magnetic inductivity and conductivity, but deprived of the electric correspondences. Thus, when g/μ is constant, the solution due to any polar field \mathbf{H}_0 is

$$\mathbf{H} = \mathbf{H}_0 \epsilon^{-\rho t}, \quad \mathbf{E} = 0; \quad . \quad . \quad . \quad (13)$$

wherein $\rho = 4\pi g/\mu$. But a solenoidal field of $\mu\mathbf{H}$ disappears at once, by instantaneous transference to infinity. Thus any varying impressed force \mathbf{h} is accompanied without delay by the corresponding steady flux, the magnetic induction.

When the inertia associated with μ is considered the result is rather striking and difficult to understand. It appears, however, to belong to the same class of (theoretical) phenomena as the following. If a coil in which there is an electric current be instantaneously shunted on to a second coil in which there is no current, then, according to Maxwell, the first coil instantly loses current and the second gains it, in such a way as to keep the momentum unchanged. Now we cannot set up a current in a coil instantly, so that we have a contradiction. But the disagreement admits of easy reconciliation. We cannot set up current instantly with a finite impressed force, but if it be infinite we can. In the case of the coils there is an electromotive impulse, or infinite electromotive force acting for an infinitely short time, when the

coils are connected, with corresponding instantaneous changes in their momenta. A loss of energy is involved.

It is scarcely necessary to remark that the true physical theory involves other considerations on account of the dielectric not being infinitely elastic and on account of diffusion in the wires ; so that we have sparking and very rapid vibrations in the dielectric. The energy which is not wasted in the spark, and which would go out to infinity were there no conducting obstacles, is probably all wasted practically in the heat of conduction-currents in them.

8. *Impressed Forces*.—Given initially \mathbf{E}_0 and \mathbf{H}_0 , we know that the diverging parts must either remain constant or subside, and are, in a manner, self-contained ; but the solenoidal fields, which would give rise to waves, may be kept from changing by means of impressed forces \mathbf{e}_0 and \mathbf{h}_0 . Thus let \mathbf{E}_0 and \mathbf{H}_0 be solenoidal. To keep them steady we have, in equations (1), (2), to get rid of $p\mathbf{E}$ and $p\mathbf{H}$. Thus

$$\left. \begin{aligned} \text{curl } (\mathbf{H}_0 - \mathbf{h}_0) &= 4\pi k \mathbf{E}_0, \\ \text{curl } (\mathbf{e}_0 - \mathbf{E}_0) &= 4\pi g \mathbf{H}_0, \end{aligned} \right\} \quad . \quad . \quad . \quad (14)$$

are the equations of steady fields \mathbf{E}_0 and \mathbf{H}_0 , these being the forces of the fluxes. Or

$$\left. \begin{aligned} \text{curl } \mathbf{h}_0 &= \text{curl } \mathbf{H}_0 - 4\pi k \mathbf{E}_0, \\ \text{curl } \mathbf{e}_0 &= \text{curl } \mathbf{E}_0 + 4\pi g \mathbf{H}_0, \end{aligned} \right\} \quad . \quad . \quad . \quad (14 a)$$

gives the curls of the required impressed forces in terms of the given fluxes, and any impressed forces having these curls will suffice.

Now, on the sudden removal of \mathbf{e}_0 , \mathbf{h}_0 , the forces \mathbf{E}_0 , \mathbf{H}_0 , which had hitherto been the forces of the fluxes, become, instantaneously, the forces of the field as well. That is, the fluxes themselves do not change suddenly, except in such a case as a tangential discontinuity in a flux produced at a surface of curl of impressed force when, at the surface itself, the mean value will be immediately assumed on removal of the impressed force. We know, therefore, the effects due to certain distributions of impressed force when we know the result of leaving the corresponding fluxes to themselves without impressed force. It is, however, the converse of this that is practically useful, viz. to find the result of leaving the fluxes without impressed force by solving the problem of the establishment of the steady fluxes when the impressed forces are suddenly started ; because this problem can often be attacked in a comparatively simple manner, requiring only investigation of the appropriate functions to suit the surfaces of curl of the

impressed forces. The remarks in this paragraph are not limited to homogeneity and isotropy.

9. *Solutions for Plane Waves.*—If we take z normal to the plane of the waves, we may suppose that both \mathbf{E} and \mathbf{H} have x and y components. This is, however, a wholly unnecessary mathematical complication, and it is sufficient to suppose that \mathbf{E} is everywhere parallel to the x -axis and \mathbf{H} to the y -axis. The specification of an initial state is therefore $\mathbf{E}_0, \mathbf{H}_0$, the tensors of \mathbf{E} and \mathbf{H} , given as functions of z ; and the equations of motion (1), (2) become

$$\left. \begin{aligned} -\frac{d\mathbf{H}}{dz} &= (4\pi k + cp)\mathbf{E}, \\ -\frac{d\mathbf{E}}{dz} &= (4\pi g + \mu p)\mathbf{H}. \end{aligned} \right\} \dots \dots (15)$$

Now the operator q^2 in (5) becomes

$$q^2 = v^2 \nabla^2 + \sigma^2; \dots \dots (16)$$

where by ∇ we may now understand d/dz simply. Therefore, by (7), the solutions of (15) are

$$\left. \begin{aligned} \mathbf{E} &= e^{-\rho t} \left[\left(\cosh qt - \frac{\sigma}{q} \sinh qt \right) \mathbf{E}_0 - \frac{\sinh qt}{q} \frac{\nabla}{c} \mathbf{H}_0 \right], \\ \mathbf{H} &= e^{-\rho t} \left[\left(\cosh qt + \frac{\sigma}{q} \sinh qt \right) \mathbf{H}_0 - \frac{\sinh qt}{q} \frac{\nabla}{\mu} \mathbf{E}_0 \right]. \end{aligned} \right\} (17)$$

When the initial states are such as $a\epsilon^{bz}$, or $a \cos bz$, the realization is immediate, requiring only a special meaning to be given to q in (17). But with more useful functions as $a\epsilon^{-bz^2}$, &c., &c., there is much work to be performed in effecting the differentiations, whilst the method fails altogether if the initial distribution is discontinuous.

But we may notice usefully that when \mathbf{E}_0 and \mathbf{H}_0 are constants the solutions are

$$\mathbf{E} = e^{-2\rho_1 t} \mathbf{E}_0, \quad \mathbf{H} = e^{-2\rho_2 t} \mathbf{H}_0, \quad \dots \dots (18)$$

which are quite independent of one another. Further, since disturbances travel at speed v , (18) represents the solutions in any region in which \mathbf{E}_0 and \mathbf{H}_0 are constant, from $t=0$ up to the later time when a disturbance arrives from the nearest plane at which \mathbf{E}_0 or \mathbf{H}_0 varies.

10. *Fourier Integrals.*—Now transform (17) to Fourier integrals. We have Fourier's theorem,

$$f(z) = \frac{1}{\pi} \int_0^\infty \int_{-\infty}^\infty f(a) \cos m(z-a) dm da, \quad \dots (19)$$

and therefore

$$\phi(\nabla^2)f(z) = \frac{1}{\pi} \int_0^\infty \int_{-\infty}^\infty f(a) \phi(-m^2) \cos m(z-a) dm da; \quad (20)$$

applying which to (17) we obtain

$$\left. \begin{aligned} E &= \frac{\epsilon^{-\rho t}}{\pi} \int_0^\infty \int_{-\infty}^\infty dm da \left[E_0 \cos m(z-a) \left(\cosh - \frac{\sigma}{q} \sinh \right) qt \right. \\ &\quad \left. + \frac{H_0}{c} m \sin m(z-a) \frac{\sinh qt}{q} \right] \\ H &= \frac{\epsilon^{-\rho t}}{\pi} \int_0^\infty \int_{-\infty}^\infty dm da \left[H_0 \cos m(z-a) \left(\cosh + \frac{\sigma}{q} \sinh \right) qt \right. \\ &\quad \left. + \frac{E_0}{\mu} m \sin m(z-a) \frac{\sinh qt}{q} \right] \end{aligned} \right\} \quad (21)$$

in which, by (16),

$$q^2 = \sigma^2 - m^2 v^2, \quad . \quad . \quad . \quad . \quad . \quad (22)$$

and E_0, H_0 are to be expressed as functions of a , whilst E and H belong to z . Discontinuities are now attackable.

The integrations with respect to m may be effected. In fact, I have done it in three different ways. First by finding the effect produced by impressed force. Secondly, by an analogous method applied to (17), transforming the differentiations to integrations. Thirdly, by direct integration of (21); this is the most difficult of all. The first method was given in a recent paper*; a short statement of the other two methods follows.

11. *Transformation of (17).*—In (17) we naturally consider the functions of qt to be expanded in rising powers of q^2 , and therefore of ∇^2 , leading to differentiations to be performed upon the initial states. But if we expand them in descending powers of ∇ , we substitute integrations, and can apply them to discontinuous initial distribution.

The following are the expansions required :—

$$\left. \begin{aligned} \frac{\epsilon^{qt}}{q} &= \frac{1}{v\nabla} \left[U_0 + U_1 \left(\frac{\sigma^2 t}{2v\nabla} \right) + \frac{U_2}{2} \left(\frac{\sigma^2 t}{2v\nabla} \right)^2 + \frac{U_3}{3} \left(\frac{\sigma^2 t}{2v\nabla} \right)^3 + \dots \right], \\ \epsilon^{qt} &= U_0 + U_0 \left(\frac{\sigma^2 t}{2v\nabla} \right) + \frac{U_1}{2} \left(\frac{\sigma^2 t}{2v\nabla} \right)^2 + \frac{U_2}{3} \left(\frac{\sigma^2 t}{2v\nabla} \right)^3 + \dots, \end{aligned} \right\} \quad (23)$$

* "Electromagnetic Waves," Phil. Mag. October 1888.

where the U 's are functions of $(v\nabla t)^{-1}$ given by

$$\left. \begin{aligned} U_0 &= \epsilon^{vt\nabla}, \quad U_1 = \epsilon^{vt\nabla} \left(1 - \frac{1}{vt\nabla} \right), \quad U_2 = \epsilon^{vt\nabla} \left(1 - \frac{3}{vt\nabla} + \frac{3}{(vt\nabla)^2} \right), \\ U_r &= \epsilon^{vt\nabla} \left[1 - \frac{r(r+1)}{2vt\nabla} + \frac{r(r^2-1^2)(r+2)}{2 \cdot 4 \cdot (vt\nabla)^2} \right. \\ &\quad \left. - \frac{r(r^2-1^2)(r^2-2^2)(r+3)}{2 \cdot 4 \cdot 6 (vt\nabla)^3} + \dots \right]; \end{aligned} \right\} \quad (24)$$

being in fact identically the same functions of $vt\nabla$ as those of r which occur in the investigation of spherical waves.

Arranged in powers of $s = \sigma/v\nabla$, we have

$$\left. \begin{aligned} \frac{\epsilon^{qt}}{q} &= \frac{\epsilon^{vt\nabla}}{v\nabla} (1 + sg_1 + s^2g_2 + \dots), \\ \epsilon^{qt} &= \epsilon^{vt\nabla} (1 + sh_1 + s^2h_2 + \dots), \end{aligned} \right\} \quad (25)$$

where

$$\left. \begin{aligned} g_1 &= \frac{\sigma t}{2}, \quad g_2 = -\frac{1}{2} + \frac{(\sigma t)^2}{2 \cdot 4}, \quad g_3 = -\frac{3}{4} \frac{\sigma t}{2} + \frac{(\sigma t)^3}{2 \cdot 4 \cdot 6}, \\ g_4 &= \frac{3}{8} - \frac{(\sigma t)^2}{2 \cdot 4} + \frac{(\sigma t)^4}{2 \cdot 4 \cdot 6 \cdot 8}, \quad g_5 = \frac{5}{8} \frac{\sigma t}{2} - \frac{5}{4} \frac{(\sigma t)^3}{2 \cdot 4 \cdot 6} + \frac{(\sigma t)^5}{2 \cdot 4 \cdot 6 \cdot 8 \cdot 10}, \\ g_6 &= -\frac{5}{16} + \frac{15}{16} \frac{(\sigma t)^2}{2 \cdot 4} - \frac{5}{2} \frac{(\sigma t)^4}{2 \cdot 4 \cdot 6 \cdot 8} + \frac{(\sigma t)^6}{2 \cdot 4 \cdot 6 \cdot 8 \cdot 10 \cdot 12}, \\ h_1 &= \frac{\sigma t}{2}, \quad h_2 = \frac{(\sigma t)^2}{2^2 \cdot 2}, \quad h_3 = \frac{1}{2^2 \cdot 2} \left(-\sigma t + \frac{(\sigma t)^3}{2 \cdot 3} \right), \\ h_4 &= \frac{1}{2^3 \cdot 3} \left(-3(\sigma t)^2 + \frac{(\sigma t)^4}{2 \cdot 4} \right), \quad h_5 = \frac{1}{2^3 \cdot 3} \left(\sigma t - \frac{6(\sigma t)^3}{2 \cdot 4} + \frac{(\sigma t)^5}{2^2 \cdot 4 \cdot 5} \right), \\ h_6 &= \frac{1}{2^4 \cdot 4} \left(15(\sigma t)^2 - \frac{10(\sigma t)^4}{2 \cdot 5} + \frac{(\sigma t)^6}{2^2 \cdot 5 \cdot 6} \right), \\ h_7 &= \frac{1}{2^4 \cdot 4} \left(-15\sigma t + \frac{45(\sigma t)^3}{2 \cdot 5} - \frac{15(\sigma t)^5}{2^2 \cdot 5 \cdot 6} + \frac{(\sigma t)^7}{2^2 \cdot 5 \cdot 6 \cdot 7} \right), \end{aligned} \right\} \quad (27)$$

The following properties of the g 's and h 's are useful. Understanding that g_0 and h_0 are unity, we have

$$\left. \begin{aligned} g_r + \sigma t g_{r+1} + \frac{(\sigma t)^2}{2} g_{r+2} + \dots &= 0 \quad \text{when } r \text{ is odd,} \\ \text{and when } r \text{ is even,} &= 1 \cdot 3 \cdot 5 \dots (r-1) (-1)^{\frac{r}{2}} \frac{J_r(\sigma t i)}{(\sigma t i)^{\frac{r}{2}}}, \\ \text{except } r=0, \text{ when} &= J_0(\sigma t i). \end{aligned} \right\} \quad (28)$$

$$h_r + \sigma t h_{r+1} + \frac{(\sigma t)^2}{2} h_{r+2} + \dots = (-1)^{r+1} \sigma t \cdot \frac{J_r(\sigma t i)}{(\sigma t i)^r}, \quad (29)$$

except when $r=0$, which case is not wanted. Now if

$$\epsilon^{qt} \left(1 + \frac{\sigma}{q} \right) = \epsilon^{vt \nabla} (1 + s f_1 + s^2 f_2 + \dots), \quad (30)$$

the f 's* will be given by (25), viz.

$$f_0 = 1, \quad f_1 = g_0 + h_1, \quad f_2 = g_1 + h_2, \text{ \&c.}; \quad (31)$$

and the properties of the f 's corresponding to (28), (29) are

$$\left. \begin{aligned} f_r + \sigma t f_{r+1} + \frac{(\sigma t)^2}{2} f_{r+2} + \dots &= \epsilon^{\sigma t} \text{ when } r=0, \\ &= 0 \text{ when } r \text{ is even, except } 0; \end{aligned} \right\} \quad (32)$$

and

$$\therefore \pm 1.3.5 \dots (r-2) \frac{J_{\frac{r-1}{2}}(\sigma t i) - i J_{\frac{r+1}{2}}(\sigma t i)}{(\sigma t i)^{\frac{r-1}{2}}}; \quad (33)$$

when r is odd, with the $+$ sign for $r=1, 5, 9, \dots$, and the $-$ sign for the rest. The first case in (32), of $r=0$, is very important. But in case $r=1$, the coefficient in (33) is $+1$; thus,

$$= (J_0 - i J_1)(\sigma t i).$$

12. *Special Initial States.*—Now let there be an initial distribution of H_0 only, so that, by (17),

$$\left. \begin{aligned} H &= \epsilon^{-\rho t} \left(\cosh + \frac{\sigma}{q} \sinh \right) q t \cdot H_0, \\ E &= -\epsilon^{-\rho t} \frac{\sinh q t}{q} \frac{\nabla}{c} H_0, \end{aligned} \right\} \quad (34)$$

by (17). Let H_0 be zero on the right side and constant on the left side of the origin, and let us find H and E at a point on the right side. The operator $\epsilon^{vt \nabla}$ is inoperative, so that, by (30),

$$\left. \begin{aligned} H &= \frac{1}{2} \epsilon^{-\rho t} \epsilon^{-vt \nabla} (1 - s f_1 + s^2 f_2 - s^3 f_3 + \dots) H_0, \\ E &= \frac{1}{2} \epsilon^{-\rho t} \epsilon^{-vt \nabla} (1 - s g_1 + s^2 g_2 - s^3 g_3 + \dots) H_0 \times \mu v, \end{aligned} \right\} \quad (35)$$

the immediate integration of which gives

* These f 's are the same as in my paper "On Electromagnetic Waves," 8, Phil. Mag. February 1888; but s there is σ here.

$$\left. \begin{aligned} H &= \frac{1}{2} H_0 \epsilon^{-\rho t} \left\{ 1 + \sigma t f_1 \left(1 - \frac{z}{vt} \right) + \frac{(\sigma t)^2}{|2|} f_2 \left(1 - \frac{z}{vt} \right)^2 + \dots \right\}, \\ E &= \frac{1}{2} \mu v H_0 \epsilon^{-\rho t} \left\{ 1 + \sigma t g_1 \left(1 - \frac{z}{vt} \right) + \frac{(\sigma t)^2}{|2|} g_2 \left(1 - \frac{z}{vt} \right)^2 + \dots \right\}. \end{aligned} \right\} \quad (36)$$

To obtain the E due to E_0 constant from $z = -\infty$ to 0, use the first of (36); change H to E , H_0 to E_0 , and change the sign of σ , not forgetting in the f 's. To obtain the corresponding H due to E_0 , use the second of (36); change E to H , H_0 to E_0 , and μ to c . So

$$\left. \begin{aligned} E &= \frac{1}{2} E_0 \epsilon^{-\rho t} \left\{ 1 - \sigma t f'_1 \left(1 - \frac{z}{vt} \right) + \frac{(\sigma t)^2}{|2|} f'_2 \left(1 - \frac{z}{vt} \right)^2 - \dots \right\}, \\ H &= \frac{1}{2} c v E_0 \epsilon^{-\rho t} \left\{ 1 + \sigma t g_1 \left(1 - \frac{z}{vt} \right) + \frac{(\sigma t)^2}{|2|} g_2 \left(1 - \frac{z}{vt} \right)^2 + \dots \right\}, \end{aligned} \right\} \quad (37)$$

where the accent means that the sign of σ is changed in the f 's.

From these, without going any further, we can obtain a general idea of the growth of the waves to the right and left of the origin, because the series are suitable for small values of σt . But, reserving a description till later, notice that E in (36) and H in (37) must be true on both sides of the origin; on expanding them in powers of z we consequently find that the coefficients of the odd powers of z vanish, by the first of (28), and what is left may be seen to be the expansion of

$$E = \frac{1}{2} \mu v H_0 \epsilon^{-\rho t} J_0 \left[\frac{\sigma}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right], \quad . \quad . \quad (38)$$

the complete solution for E due to H_0 . Similarly,

$$H = \frac{1}{2} c v E_0 \epsilon^{-\rho t} J_0 \left[\frac{\sigma}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right] \quad . \quad . \quad . \quad (39)$$

is the complete solution for H due to E_0 . In both cases the initial distribution was on the left side of the origin; but, if its sign be reversed, it may be put on the right side, without altering these solutions.

Similarly, by expanding the first of (36) and first of (37) in powers of z we get rid of the even powers of z , and produce the solutions given by me in a previous paper*, which, however, it is needless to write out here, owing to the complexity.

13. *Arbitrary Initial States*.—Knowing the solutions due to the above distributions, we find those due to initial $E_0 da$ at the origin, or $H_0 da$, by differentiation to z ; and for this we do not need the firsts of (36) and (37) but only the seconds.

* "Electromagnetic Waves," § 8 (Phil. Mag. Feb. 1888).

The results bring the Fourier integrals (21) to

$$\left. \begin{aligned} E &= \epsilon^{-\rho t} \left[\frac{1}{2} (E_0 + \mu v H_0)_{z-vt} + \frac{1}{2} (E_0 - \mu v H_0)_{z+vt} \right. \\ &\quad \left. + \frac{1}{2} \int_{z-vt}^{z+vt} \left\{ E_0 \frac{-\sigma+p}{v} - \frac{H_0}{cv} \nabla \right\} J_0(y) da \right], \\ H &= \epsilon^{-\rho t} \left[\frac{1}{2} (H_0 + cv E_0)_{z-vt} + \frac{1}{2} (H_0 - cv E_0)_{z+vt} \right. \\ &\quad \left. + \frac{1}{2} \int_{z-vt}^{z+vt} \left\{ H_0 \frac{\sigma+p}{v} - \frac{E_0}{\mu v} \nabla \right\} J_0(y) da \right]; \end{aligned} \right\} \quad (40)$$

where

$$p = d/dt, \quad \nabla = d/dz, \quad y = \frac{\sigma}{v} \{ (z-a)^2 - v^2 t^2 \}^{\frac{1}{2}}.$$

Another interesting form is got by the changes of variables

$$\left. \begin{aligned} U &= \frac{1}{2} \epsilon^{\rho t} (E - \mu v H), \quad u = z - vt, \\ W &= \frac{1}{2} \epsilon^{\rho t} (E + \mu v H), \quad w = z + vt. \end{aligned} \right\} \quad (41)$$

These lead to

$$\left. \begin{aligned} U_{z,t} &= U_{w,0} + \int_u^w \left(U_0 \frac{d}{dw} - \frac{\sigma}{2v} W_0 \right) J_0 \left\{ \frac{\sigma}{v} (u-a)^{\frac{1}{2}} (w-a)^{\frac{1}{2}} \right\} da, \\ W_{z,t} &= W_{u,0} - \int_u^w \left(W_0 \frac{d}{du} + \frac{\sigma}{2v} U_0 \right) J_0 \left\{ \frac{\sigma}{v} (u-a)^{\frac{1}{2}} (w-a)^{\frac{1}{2}} \right\} da. \end{aligned} \right\} \quad (42)$$

The connexions and partial characteristic of U or W are

$$\frac{dW}{dw} = -\frac{\sigma}{2v} U, \quad \frac{dU}{du} = +\frac{\sigma}{2v} W, \quad \frac{d^2 U}{du dw} = -\left(\frac{\sigma}{2v}\right)^2 U; \quad (43)$$

and this characteristic has a solution

$$U = \left(\frac{z+vt}{z-vt} \right)^{\frac{m}{2}} J_m \left[\frac{\sigma}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right], \quad (44)$$

where m is any + integer, and in which the sign of the exponent may be reversed. We have utilized the case $m=0$ only.

14. *Evaluation of Fourier Integrals.*—The effectuation of the integration (direct) of the original Fourier integrals will be found to ultimately depend upon

$$\frac{2}{\pi} \int_0^{\infty} \cos mz \frac{\sinh qt}{q} dm = \frac{1}{v} J_0 \left[\frac{\sigma}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right], \quad (45)$$

provided $vt > z$, where, as before,

$$q^2 = \sigma^2 - m^2 v^2.$$

By equating coefficients of powers of z^2 in (45) we get

$$\frac{2}{\pi} \int \frac{\sinh qt}{q} m^{2r} dm = \frac{1 \cdot 3 \cdot 5 \cdot (2r-1)}{v^{2r+1}} \frac{J_r(\sigma t i)}{(\sigma t i)^r}, \quad (46)$$

except with $r=0$; then

$$= \frac{1}{v} J_0(\sigma t i).$$

To prove (45), expand the q function in powers of σ^2 . Thus, symbolically written,

$$\frac{\sinh qt}{q} = \epsilon^{\frac{1}{2}\sigma^2 p^{-1}t} \left(\frac{\sin mvt}{mv} \right), \quad \dots \quad (47)$$

the operand being in the brackets, and p^{-1} meaning integration from 0 to t with respect to t . Thus, in full,

$$\begin{aligned} \frac{2}{\pi} \int \cos mz \frac{\sinh qt}{q} dm &= \frac{2}{\pi} \int_0^\infty \cos mz \left[\frac{\sin mvt}{mv} + \frac{\sigma^2}{2} \int_0^t t \frac{\sin mvt}{mv} dt \right. \\ &\quad \left. + \frac{\sigma^2}{2} \frac{\sigma^2}{4} \int_0^t t dt \int_0^t t dt \frac{\sin mvt}{mv} + \dots \right] dm. \quad (48) \end{aligned}$$

Now the value of the first term on the right is

$$\frac{1}{v}, \quad \frac{2}{v}, \quad \text{or } 0,$$

when z is $<$, $=$, or $> vt$.

Thus, in (48) if $z > vt$, since first term vanishes, so do all the rest, because their values are deduced from that of the first by integrations to t , which during the integrations is always $< z/v$. Therefore the value of the left number of (45) is zero when $z > vt$. In another form, disturbances cannot travel faster than at speed v .

But when $z < vt$ in (48), it is clear that whilst t' goes from 0 to t or from 0 to z/v , and then from z/v to t , the first integral is zero from 0 to z/v , so that the part z/v to t only counts. Therefore the second term is

$$\begin{aligned} \frac{2}{\pi} \frac{\sigma^2}{2} \int \cos mz \left[\int_{\frac{z}{v}}^t t \frac{\sin mvt}{mv} dt \right] dm &= \frac{2}{\pi} \frac{\sigma^2}{2} \int_{\frac{z}{v}}^t t dt \int_0^\infty \cos mz \frac{\sin mvt}{mv} dm \\ &= \frac{\sigma^2}{2} \frac{1}{v} \int_{\frac{z}{v}}^t t dt = \frac{1}{v} \frac{\sigma^2}{2^2} \left(t^2 - \frac{z^2}{v^2} \right). \end{aligned}$$

The third is, similarly,

$$\frac{1}{v} \frac{\sigma^2}{2^2} \frac{\sigma^2}{4} \int_{\frac{z}{v}}^t t \left(t^2 - \frac{z^2}{v^2} \right) dt = \frac{1}{v} \frac{\sigma^4}{2^2 4^2} \left(t^2 - \frac{z^2}{v^2} \right)^2;$$

and so on, in a uniform manner, thus proving that the successive terms of (48) are the successive terms of the expansion of (45) (right number) in powers of σ^2 ; and therefore proving (45).

The following formulæ occur when the front of the wave is in question, where caution is needed in evaluations:—

$$\cosh \sigma t - \frac{1}{2} = \frac{2}{\pi} \int_0^\infty \frac{\sin mvt}{m} \cosh qt \, dm, \quad . \quad . \quad (49)$$

$$\frac{\sinh \sigma t}{\sigma} = \frac{2}{\pi} \int_0^\infty \frac{\sin mvt}{m} \frac{\sinh qt}{q} \, dm. \quad . \quad . \quad (50)$$

15. *Interpretation of Results.*—Having now given a condensation of the mathematical work, we may consider, in conclusion, the meaning and application of the formulæ. In doing so, we shall be greatly assisted by the elementary theory of a telegraphic circuit. It is not merely a mathematically analogous theory, but is, in all respects save one, essentially the same theory, physically, and the one exception is of a remarkable character. Let the circuit consist of a pair of equal parallel wires, or of a wire with a coaxial tube for the return, and let the medium between the wires be slightly conducting. Then, if the wires had no resistance, the problem of the transmission of waves would be the above problem of plane waves in a real dielectric, that is, with constants μ , c , and k , but without the magnetic conductivity; i. e. $g=0$ in the above.

The fact that the lines of magnetic and electric force are no longer straight is an unessential point. But it is, for convenience, best to take as variables, not the forces, but their line-integrals. Thus, if V be the line-integral of E across the dielectric between the wires, V takes the place of E . Thus kE , the density of the conduction-current, is replaced by KV , where K is the conductance of the dielectric per unit length of circuit, and $cE/4\pi$, the displacement, becomes SV , where S is the permittance per unit length of circuit. The density of electric current $cpE/4\pi$ is then replaced by SpV . Also SV is the charge per unit length of circuit.

Next, take the line-integral of $H/4\pi$ round either conductor for magnetic variable. It is C , usually called the current in the wires. Then μH , the induction, becomes LC ; where LC is the momentum per unit length of circuit, L being the inductance, such that $LSv^2 = \mu cv^2 = 1$.

A more convenient transformation (to minimize the trouble with 4π 's) is

$$\begin{array}{l} E \text{ to } V, \quad \mu \text{ to } L, \quad c \text{ to } S, \quad 4\pi k \text{ to } K. \\ H \text{ to } C, \end{array}$$

Now, lastly, the wires have resistance, and this is without any representation whatever in a real dielectric. But, as I have before shown, the effect of the resistance of the wires in attenuating and distorting waves is, to a first approximation (ignoring the effects of imperfect penetration of the magnetic field into the wires), representable in the same manner exactly as the corresponding effects due to g , the hypothetical magnetic conductivity of a dielectric*. Thus, in addition to the above,

$$4\pi g \text{ becomes } R,$$

R being the resistance of the circuit per unit length.

16. In the circuit, if infinitely long and perfectly insulated, the total charge is constant. This property is independent of the resistance of the wires. If there be leakage, the charge Q at time t is expressed in terms of the initial charge Q_0 by

$$Q = Q_0 e^{-Kt/S},$$

independent of the way the charge redistributes itself.

In the general medium, the corresponding property is persistence of displacement, no matter how it redistributes itself, provided k be zero, whatever g may be. And, if there be electric conductivity,

$$\int_{-\infty}^{\infty} D \, dz = \left(\int_{-\infty}^{\infty} D_0 \, dz \right) e^{-4\pi k t/c},$$

where D_0 is the initial displacement, and D that at time t , functions of z .

In the circuit, if the wires have no resistance, the total momentum remains constant, however it may redistribute itself. This is an extension of Maxwell's well-known theory of a linear circuit of no resistance. The conductivity of the dielectric makes no difference in this property, though it causes a loss of energy. When the wires have resistance, then

$$\int_{-\infty}^{\infty} LC \, dz = \left(\int_{-\infty}^{\infty} LC_0 \, dz \right) e^{-Rt/L}$$

expresses the subsidence of total momentum; and this is independent of the manner of redistribution of the magnetic field and of the leakage.

In the general medium, when real, the corresponding property is persistence of the induction (or momentum); and when g is finite,

$$\int_{-\infty}^{\infty} \mu H \, dz = \left(\int_{-\infty}^{\infty} \mu H_0 \, dz \right) e^{-4\pi g t/\mu}.$$

* "Electromagnetic Waves," § 6 (Phil. Mag. Feb. 1888).

In passing, I may remark that, in my interpretation of Maxwell's views, it is not his vector-potential \mathbf{A} , the so-called electrokinetic momentum, that should have the physical idea of momentum associated with it, but the magnetic induction \mathbf{B} . To illustrate, consider Maxwell's theory of a linear circuit of no resistance, the simplest case of persistence of momentum. We may express the fact by saying that the induction through the circuit remains constant, or that the line-integral of \mathbf{A} along or in the circuit remains constant. These are perfectly equivalent. Now if we pass to an infinitely small closed circuit, the line-integral of \mathbf{A} becomes \mathbf{B} itself (per unit area). But if we consider an element of length only, we get lost at once.

Again, the magnetic energy being associated with \mathbf{B} , (and \mathbf{H}), so should be the momentum.

Suppose also we take the property that the line-integral of $-\dot{\mathbf{A}}$ is the E.M.F. in a circuit, and then consider $-\dot{\mathbf{A}}$ as the electric force of induction at a point. Its time-integral is \mathbf{A} . But this is an electromotive impulse, not momentum.

Lastly, whilst \mathbf{B} (or \mathbf{H}) defines a physical property at a point, \mathbf{A} does not, but depends upon the state of the whole field, to an infinite distance. In fact it sums up, in a certain way, the effect which would arise at a point from disturbances coming to it from all parts of the field. It is therefore, like the scalar electric potential, a mathematical concept merely, not indicative in any way of the actual state of the medium anywhere.

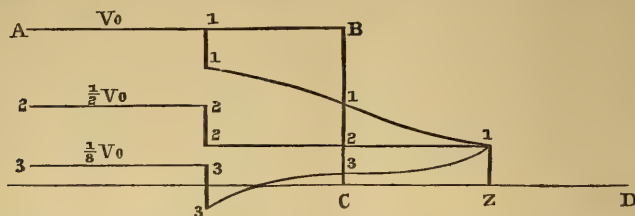
The time-integral of \mathbf{H} , whose curl is proportional to the displacement, has equal claims to notice as a mathematical function which is of occasional use for facilitating calculations, but which should not, in my opinion, be elevated to the rank of a fundamental quantity, as was done by Maxwell with respect to \mathbf{A} .

Independently of these considerations, the fact that \mathbf{A} has often a scalar potential parasite, and also the other function, causes sometimes great mathematical complexity and indistinctness; and it is, for practical reasons, best to murder the whole lot, or at any rate merely employ them as subsidiary functions.

17. Returning to the telegraph-circuit, let the initial state be one of uniform V on the whole of the left side of the origin, $V=0$ on the right side, and $C=0$ everywhere. The diagram will serve to show roughly what happens in the three principal cases.

First of all we have ABCD to represent the curve of V_0 , the origin being at C. When the disturbance has reached Z,

that is when $t = CZ/v$, the curve is A 1 1 1 1 Z if there be no leakage, when R and L are such that $\epsilon^{-Rt} = \frac{1}{2}$. At the



origin $V = \frac{1}{2}V_0$, at the front $V = \frac{1}{4}V_0$, and at the back $V = \frac{3}{4}V_0$.

Now introduce leakage to make $R/L = K/S$. Then 2 2 2 2 1 Z shows the curve of V , provided $\epsilon^{-Kt/S} = \frac{1}{2}$. We have $V = \frac{1}{2}V_0$ on the left, and $V = \frac{1}{4}V_0$ in the rest.

Thirdly, let the leakage be in excess. Then, when V_0 has fallen, by leakage only, to $\frac{1}{8}V_0$ on the left, the curve 3 3 3 3 1 Z shows V ; it is $\frac{1}{16}V_0$ at the origin, $-\frac{1}{8}V_0$ at the back, and $\frac{1}{4}V_0$ at the front.

Of course there has to be an adjustment of constants to make $\epsilon^{-(\frac{R}{2L} + \frac{K}{2S})t}$ be the same $\frac{1}{2}$ in all cases, viz. the attenuation at the front.

18. Precisely the same applies when it is C_0 that is initially given instead of V_0 , provided we change the sign of σ . That is, we have the curve 1 when the leakage is in excess, and the curve 3 when the leakage is smaller than that required to produce nondistortional transmission.

19. Now transferring attention to the general medium, if we make the substitution of magnetic conductivity for the resistance of the wires, the curve 1 would apply when it is E_0 that is the initial state and g in excess, and 3 when it is deficient; whilst if H_0 is the initial state, 1 applies when g is deficient, and 3 when in excess. But g is really zero, so we have the curve 1 for that of H and 3 for that of E .

This forcibly illustrates the fact that the diffusion of charge in a submarine cable and the diffusion of magnetic disturbances in a good conductor, though mathematically analogous, are physically quite different. They are both extreme cases of the same theory; but they arise by going to opposite extremities; with the peculiar result that, whereas the time-constant of retardation in a submarine cable is proportional to the resistance of the wire, that in the wire itself is proportional to its conductivity.

20. Going back to the diagram, if we shift the curves bodily

through unit distance to the left, and then take the difference between the new and the old curves, we shall obtain the curves showing how an initial distribution of V or C through unit-distance at the origin divides and spreads. In the case of curve 2, we have clean splitting without a trace of diffusion. In the other cases there is a diffused disturbance left behind between the terminal waves, positive in case 1, negative in case 3. But I have sufficiently described this matter in a former paper*.

October 18, 1888.

POSTSCRIPT.

On the Metaphysical Nature of the Propagation of the Potentials.

At the recent Bath Meeting of the British Association there was considerable discussion† in Section A on the question of the propagation of electric potential. I venture therefore to think that the following remarks upon this subject may be of interest.

According to the way of regarding the electromagnetic quantities I have consistently carried out since January 1885, the question of the propagation of, not merely the electric potential Ψ , but the vector potential \mathbf{A} , does not present itself as one for discussion; and, when brought forward, proves to be one of a metaphysical nature.

We make acquaintance, experimentally, not with potentials, but with forces, and we formulate observed facts with the least amount of hypothesis, in terms of the electric force \mathbf{E} and magnetic force \mathbf{H} . In Maxwell's development of Faraday's views, \mathbf{E} and \mathbf{H} actually represent the state of the medium anywhere. (It comes to the same thing if we consider the fluxes, but less conveniently in general.) Granting this, it is perfectly obvious that in any case of propagation, since it is a physical state that is propagated, it is \mathbf{E} and \mathbf{H} that are propagated.

Now, in a limited class of cases, \mathbf{E} is expressible as $-\nabla\Psi$. Considerations of mathematical simplicity alone then direct the mathematician's attention to Ψ and its investigation, rather than to that of \mathbf{E} directly. But when this is possible the field is steady, and no question of propagation presents itself

* "Electromagnetic Waves," § 7 (Phil. Mag. Feb. 1888).

† See Prof. Lodge's "Sketch of the Electrical Papers read in Section A," the 'Electrician,' September 21 and 28, 1888.

(except in the very artificial form of balanced exchanges). When there is propagation, and \mathbf{H} is involved, we have

$$\mathbf{E} = -\nabla\psi - \dot{\mathbf{A}}.$$

Now this is, not an electromagnetic law specially, but strictly a truism, or mathematical identity. It becomes electromagnetic by the definition of \mathbf{A} ,

$$\text{curl } \mathbf{A} = \mu\mathbf{H},$$

leaving \mathbf{A} indeterminate as regards a diverging part, which, however, we may merge in $-\nabla\psi$. Supposing, then, \mathbf{A} and ψ to become fixed in this or some other way, the next question in connexion with propagation is, Can we, instead of the propagation of \mathbf{E} and \mathbf{H} , substitute that of ψ and \mathbf{A} , and obtain the same knowledge, irrespective of the artificiality of ψ and \mathbf{A} ? The answer is perfectly plain—we cannot do so. We could only do it if ψ , \mathbf{A} , given everywhere, found \mathbf{E} and \mathbf{H} . But they cannot. \mathbf{A} finds \mathbf{H} , irrespective of ψ , but both together will not find \mathbf{E} . We require to know a third vector, $\dot{\mathbf{A}}$. Thus we have ψ , \mathbf{A} , and $\dot{\mathbf{A}}$ required, involving *seven* scalar specifications to find the *six* in \mathbf{E} and \mathbf{H} . Of these three quantities, the utility of \mathbf{A} is simply to find \mathbf{H} , so that we are brought to a highly complex way of representing the propagation of \mathbf{E} in terms of ψ and $\dot{\mathbf{A}}$, giving no information about \mathbf{H} , which is, it seems to me, as complex and artificial as it is useless and indefinite.

Again, merely to emphasize the preceding, the variables chosen should be capable of representing the energy stored. Now the magnetic energy may be expressed in terms of \mathbf{A} , though with entirely erroneous localization; but the electric energy cannot be expressed in terms of ψ . Maxwell (chap. xi. vol. ii.) did it, but the application is strictly limited to electrostatics; in fact, Maxwell did not consider electric energy comprehensively. The full representation in terms of potentials requires ψ and \mathbf{Z} , the vector-potential of the magnetic current. [This is developed in my work "On Electromagnetic Induction and its Propagation," *Electrician*, 1885.] This inadequacy alone is sufficient to murder ψ and \mathbf{A} , considered as subjects of propagation.

Now take a concrete example, leaving the abstract mathematical reasoning. Let there be first no \mathbf{E} or \mathbf{H} anywhere. To produce any, impressed force is absolutely needed. Let it be impressed \mathbf{e} , and of the simplest type, viz. an infinitely extended plane sheet of \mathbf{e} of uniform intensity, acting normally to the plane. What happens? Nothing at all. Yet

the potential on one side of the plane is made greater by the amount e (tensor of \mathbf{e}) than on the other side. Say $\Psi = \frac{1}{2}e$ and $-\frac{1}{2}e$. Thus we have *instantaneous* propagation of Ψ to infinity. I prefer, however, to say that this is only a mathematical fiction, that nothing is propagated at all, that the electromagnetic mechanism is of such a nature that the applied forces are balanced *on the spot*, that is, in the sheet, by the reactions.

To emphasize this again, let the sheet be not infinite, but have a circular boundary. Let the medium be of uniform inductivity μ , and permittivity c . Then, irrespective of its conductivity, disturbances are propagated at speed $v = (\mu c)^{-\frac{1}{2}}$, and their source is the vortex-line of \mathbf{e} , on the edge of the disk. At any time t less than a/v , where a is the radius of the disk, the disturbance is confined within a ring whose axis is the vortex-line. Everywhere else $\mathbf{E} = 0$ and $\mathbf{H} = 0$. On the surface of the ring, $\mathbf{E} = \mu v \mathbf{H}$, and \mathbf{E} and \mathbf{H} are perpendicular; there can be no normal component of either.

Now we can naturally explain the absence of any flux in the central portion of the disk by the applied forces being balanced by the reactions on the spot, until the wave arrives from the vortex-line. But how can we explain it in terms of Ψ , seeing that Ψ has now to change by the amount e at the disk, and yet be continuous everywhere else outside the ring? We cannot do it, so the propagation of Ψ fails altogether. Yet the actions involved must be the same whether the disk be small or infinitely great. We must therefore give up the idea altogether of the propagation of a Ψ to balance impressed force. In the ring itself, however, we may regard the propagation of Ψ (a different one), \mathbf{A} , and $\dot{\mathbf{A}}$; or, more simply, of \mathbf{E} and \mathbf{H} .

If there be no conductivity, the steady electric field is assumed anywhere the moment the two waves from opposite ends of a diameter of the disk coexist; that is, as soon as the wave arrives from the more distant end (Phil. Mag. May 1888*). But this simplicity is quite exceptional, and seems to be confined to plane and spherical waves. In general there is a subsidence to the steady state after the initial phenomena.

If it be remarked that incompressibility (or something equivalent or resembling it) is needed in order that the medium may behave as described (*i. e.* no flux except at the vortex-line initially), and that if the medium be compressible we shall have other results (a pressural wave, for example, from the disk generally), the answer is that this is a wholly inde-

* "Electromagnetic Waves," § 25.

pendent matter, not involved in Maxwell's dielectric theory, though perhaps needing consideration in some other theory. But the moment we let the electric current have divergence (the absence of which makes the vortex-lines of \mathbf{e} to be the sources of disturbances), we at once (in my experience) get lost in an almost impenetrable fog of potentials. Maxwell's theory unamended, on the other hand, works perfectly and without a trace of indefiniteness, provided we regard \mathbf{E} and \mathbf{H} as the variables, and discard his "equations of propagation" containing the two potentials.

October 22, 1888.

V. *On the Upper Limit of Refraction in Selenium and Bromine.*
By REV. T. PELHAM DALE, M.A.*

IN my former paper read before the Society † I showed that the value of the limit could be found by the solution of the equation

$$a \sin \theta = \sin m\theta;$$

where a is the ratio of wave-lengths in free æther, and $\theta = \frac{\pi h}{l}$, h being what I there called the molecular distance, and l the corresponding wave-length within the medium. If $a \sin \theta$ be greater than unity, the solution is imaginary.

It was also shown that if ν be limit of refraction, μ the index corresponding to θ , that

$$\frac{\mu \sin \theta}{\theta} = \nu.$$

If $\theta = \frac{\pi}{2}$, we have

$$\mu = \frac{\pi}{2} \nu;$$

μ being the index of the limit of refraction towards the violet end of the spectrum. Call this the upper limit, and denote it by μ_k .

∴ $\mu_k - \nu$ is the total dispersion

$$\text{also} \quad = \left(\frac{\pi}{2} - 1 \right) \nu;$$

$$\frac{\mu_k - 1}{d} = \text{constant independent of temperature.}$$

Also by the relation

$$\frac{\pi h}{l} = \theta,$$

* Communicated by the Physical Society: read November 10, 1888.

† Phil. Mag. May 1888, p. 325.

we have for l_k the upper limit

$$\frac{\pi h}{l_k} = \frac{\pi}{2},$$

or

$$2h = l_k.$$

Hence the quantity h is half the wave-length within the medium of the limiting transmissible ray.

As also

$$\sin \theta = \frac{\lambda}{\lambda_1} \sin \theta_1,$$

or

$$\frac{\lambda}{\lambda_1} = a$$

we have at the limit

$$\begin{aligned} \frac{\lambda}{\lambda_k} \sin \theta &= \sin m\theta, \\ &= \sin \frac{\pi}{2} = 1; \end{aligned}$$

$$\therefore \lambda \sin \theta = \lambda_k.$$

If on further examination this upper limit should be established as a physical entity it will modify our ideas of dispersion, which we must then regard as identical with refraction.

There is a relation between the Eulerian integral $\Gamma(n)$ and $\frac{\sin \theta}{\theta}$ which is worthy of notice,

$$n\pi = \theta.$$

By a well-known relation,

$$\Gamma n \cdot \Gamma 1 - n = \frac{\pi}{\sin n\pi}.$$

Hence

$$\Gamma(1+n)\Gamma(1-n) = \frac{n\pi}{\sin n\pi}.$$

If a table of $\Gamma(1+n)$ is at hand, it can be used as a practically expeditious method of finding the value denoted by θ^* .

Also if for n we write $1-n$ in the above, we have

$$\Gamma(1+1-n)(\Gamma 1 - (1-n)) = \frac{\pi - n\pi}{\sin(\pi - n\pi)},$$

or

$$\frac{1-n}{n} \Gamma(1+n) \Gamma(1-n) = \frac{\pi - \theta}{\sin(\pi - \theta)},$$

* The tables of logarithms by Vassal containing a column of circular measures of arc serve every purpose as far as calculation is concerned.

which gives us

$$\frac{\sin(\pi - \theta)}{\pi - \theta} = \frac{n}{1 - n} \cdot \frac{\sin \theta}{\theta}.$$

Now since $\sin(\pi - \theta)$ is equally a solution of the equation, $a \sin \theta = \sin m\theta$, it is probable therefore that this solution may have a physical meaning.

It was noticed in my former paper that, of the substances for which data were at hand, selenium alone had its upper limit within the visible spectrum. Accordingly a fresh calculation of the values of θ_k , ν , and λ_k were made and the results given below (Table I.).

It will be seen from the table that the limiting wave-length which is transmissible is $\lambda = 5295.7$. This is a little below λ_E , which is 5269.13. It was of great interest to observe how far this purely theoretical result would be supported by observation. As neither temperature nor specific gravity of specimen were given, and the indices of refraction to the third place of decimals only, it was evident that, as a preliminary experiment, all that could be sought for was a general agreement.

With this in view I had a Browning amateur spectroscopy-eyepiece fitted to my microscope, an old instrument constructed many years ago by Troughton and Sims. After a few trials I succeeded in obtaining a tolerably uniform film of selenium mounted as an ordinary microscope-slide. The film when cold was quite transparent, and transmitted a deep ruby tint. This film was found under the spectroscopy to transmit rays nearly to the line D. Another and thinner slide was prepared. Under the microscope the thinner portions were found to be of a more orange tint, due to a spectrum reaching to the beginning of the green. Even in brilliant sunlight no sensible extension beyond this took place. In parts of the film were holes. If these were in the field they appeared as spectra extending to the violet end, but with sharp well-defined sides, showing apparently that a very thin film was effective to stop all the upper rays. Thus, then, the agreement of theory and observation seems as satisfactory as could be anticipated, considering both the data and instrumental means employed.

The examination of sulphur gave similar results. Here the limiting wave-length lies beyond the visible spectrum. A film of melted sulphur transmits yellow light. This film darkens rapidly as the temperature rises, and when the sulphur begins to boil, it, as is well known, assumes a brownish tint. Examined under the spectro-microscope, it is seen that

the absorption is confined to the violet end. If the film be placed boiling under the spectro-microscope, the spectrum is seen to be generally darkened till near the red end, and the violet quite cut off. As the film cools, the resulting dark cloud seems to recede towards the violet. If the film be very thin it appears colourless to the eye, and under the spectro-scope the violet is visible. It remains colourless after the sulphur has crystallized, appearing nearly white to the eye. Selenium also darkens rapidly by heat and then transmits a greyish light, which is probably due to its breaking up into crystals. It has, however, the property of becoming more transparent as it cools. Thus the optical properties of these two substances are as remarkably similar as are their chemical relations.

It will be observed that the quantity $\frac{\nu-1}{d}$, though by no means the same in both selenium and sulphur, are not far apart. There is, however, another relation which exists; it is expressed by the formula, that in selenium,

$$\nu_{\text{Se}} - 1 = (\nu_{\text{S}} - 1) \frac{E}{d};$$

where ν_{S} is limit of refraction of sulphur, E its equivalent, and d its density. That is, that the refraction-equivalent of selenium can be found by multiplying the refraction-equivalent of sulphur by the equivalent of selenium divided by the density of that element. This is worthy of remark, as it appears that the same relation within five or six units in the second place of decimals between isobutyl iodide as compared with isobutyl chloride, and orthobromotoluene compared with chlorotoluene and benzyl chloride. With regard to benzyl chloride, I may mention that calculation for the upper limit has revealed an error due to extracting the wrong logarithm of $\sin \theta_{\text{A}}$. The correct result given below agrees better with the rule that $\frac{\nu-1}{d}$ is constant in isomeric bodies. As in the case of two isomeric bodies, the equivalent is the same in both, it will be seen that this result is in agreement with the relation between selenium and sulphur stated above.

It is worthy of note that chlorine and bromine resemble sulphur and selenium in tint of transmitted light. The index of refraction for the line A in bromine is $\mu_{\text{A}} = 1.6260$. It occurred to me that it would be possible to obtain a probable idea of the spectrum of this element by assuming an upper limit of refraction. Taking this limit as D and F, to which I subsequently added G, I found that F gave $\theta_{\text{A}} = 39^{\circ} 44'$,

$\nu = 1.4988$, and the refraction-equivalent 13.1 . Dr. Gladstone, calculating from A, gives as a probable value 15.3 , which would necessarily be greater than that obtained from the limit. A refraction-equivalent for ν obtained from orthobromotoluine showed that for this substance the refraction-equivalent for ν was between 14 and 15 . If we assume G as the limit, then bromine comes out as 13.7 . I subsequently procured a specimen of bromine. In as thin a film as I could produce, the spectra of bromine and selenium were almost indistinguishable; but the tint of bromine is decidedly more orange, and its vapour transmitted rays up to F certainly, and perhaps beyond; but in the brightest sunlight, and even when mixed with air, there was no indication of violet rays. So far as these very imperfect observations extend, they seem to support the conclusions previously arrived at by calculation. Without venturing, then, to speak at all decisively, the subject seems a promising one and likely to repay further investigation.

In the absence of data as to the spectrum of liquid chlorine, it is useless to make calculations as to any relations which might be found to subsist between it and bromine. It is, however, of importance to observe that its yellow tint is so similar to that of sulphur, that it is not improbable that the same portions of the spectrum would be affected in both. If this on further examination should turn out to be the fact, it would be one step more towards the attainment of the form of the function $\mu = \phi(\lambda)$, which would probably reveal relations of great interest toward determining the chemical constitution of bodies.

In the case of the critical angle the imaginary sine corresponds with the change from refraction to total reflexion. And this, again, to alteration of phase in the wave of the polarized ray. Is there anything similar in the limit of refraction? I think there is. If a mirror be constructed by melting selenium on a glass slide and then pressing on it a somewhat cooler glass, we shall obtain a reflecting surface on removing the glass, and also another on the corresponding surface attached to the glass. At angles near perpendicular incidence the reflected light has a certain greenish tint. This is more apparent on the side next the glass than on the free reflecting surface, but is very evident in both. It is no doubt to be expected that light not transmitted should in certain cases be reflected. If it be absorbed there must be an expenditure of energy within the medium, which we have every reason to believe acts on the wave-length within the medium and the velocity of its transmission. The importance of selenium is that it is a manageable substance, in which the critical point

of transmission lies near the middle of the luminous spectrum. Bromine would no doubt prove equally interesting, but is by no means a pleasant substance to handle.

I should hardly have ventured to come before the Society with these very rough experiments, had I not hoped that some who possessed the instrumental means would be induced to make the observations. For my own part I am more than content with the humble though somewhat laborious office of computer between mathematical investigator and observer.

TABLE I.—Comparison of Sulphur and Selenium.

Selenium.						
Density. d .	Lower index. μ_{Δ} .	Upper index. μ_{∇} .	Arc. θ_{Δ} .	Lower limit. ν	$\frac{\nu-1}{d}$.	Shortest wave- length. λ_k .
4.3	2.653	2.980	44° 9' 0"	2.3976	32502	5295.7
Sulphur						
* d .	μ_{Δ} .	μ_{∇} .	θ_{Δ} .	ν .	$\frac{\nu-1}{d}$.	
d' 2.07 d'' 1.98	} 1.9024	1.9527	20° 32' 0"	1.8610 {	41648	
					43535	

* d' is the density of native sulphur.
 d'' " " of that from fusion.

Thus we have the following empirical relation. If E be the equivalent of sulphur, and ν its limit of refraction,

$$\frac{\nu-1}{d'} E = 13326,$$

$$\frac{\nu-1}{d''} = 13932.$$

Again, we have

$$\text{Orthobromotoluine,} \quad \frac{\nu-1}{d} = 5299,$$

$$\text{Ref. equiv. Benzyl chloride} \quad \frac{\nu-1}{d} E = 5941.7,$$

$$\text{do. do. Chlorotoluine} \quad \frac{\nu-1}{d} E = 5907.2,$$

Isobutyl iodide, $\nu-1 = 4749$,
Ref. equiv. Isobutyl chloride, $\frac{\nu-1}{d} E = 4163.9$.

TABLE II.—Bromine.

Spec. grav.	μ_A .	Assumed upper limit.		
		μ_D .	μ_F .	μ_G .
3.085	1.6260	give 10.9 corresponding refraction-equivalents.	13.1	13.7

Note on these Tables.—They can only be accepted as approximate, but the calculations have been carried quite as far as the uncertainty of the original data warrant. Under these circumstances the coincidences noticed between sulphur and selenium are of no great value, except perhaps as pointing out a likely direction in which the search for these empirical relations amongst the refractive indices should be pursued.

VI. *On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix.* By Prof. J. V. JONES, M.A., Principal of the University College, Cardiff*.

IF M is the coefficient of mutual induction of any two curves, we have

$$M = \iint \frac{\cos \epsilon}{r} ds ds';$$

where r = the distance between two elements ds and ds' , and ϵ = the angle between them.

Let us take for the equations of the circle and helix :—

$$\left. \begin{aligned} y &= a \cos \theta, \\ z &= a \sin \theta, \end{aligned} \right\} \dots \dots \dots (i.)$$

$$\left. \begin{aligned} y' &= A \cos \theta', \\ z' &= A \sin \theta', \\ x' &= k\theta'. \end{aligned} \right\} \dots \dots \dots (ii.)$$

* Communicated by the Physical Society: read November 10, 1888.

Then

$$M = \iint \frac{dx dx' + dy dy' + dz dz'}{\sqrt{(x-x')^2 + (y-y')^2 + (z-z')^2}}$$

$$= \int_0^{2\pi} \int_0^\Theta \frac{Aa \cos(\theta - \theta') d\theta d\theta'}{\sqrt{A^2 + a^2 - 2Aa \cos(\theta - \theta') + k^2\theta'^2}}.$$

If we change the variables in this integral, putting

$$\theta - \theta' = \phi,$$

$$\theta' = \phi',$$

we find

$$M = \int_{-\Theta}^{2\pi - \Theta} \int_{-\phi}^\Theta V d\phi d\phi' + \int_{2\pi - \Theta}^0 \int_{-\phi}^{2\pi - \phi} V d\phi d\phi' + \int_0^{2\pi} \int_0^{2\pi - \phi} V d\phi d\phi', \quad (I.)$$

where

$$V = \frac{Aa \cos \phi}{\sqrt{A^2 + a^2 - 2Aa \cos \phi + k^2\phi'^2}}$$

$$= \frac{Aa \cos \phi}{\sqrt{\alpha^2 + k^2\phi'^2}},$$

if

$$\alpha^2 = A^2 + a^2 - 2Aa \cos \phi,$$

$$= \frac{Aa \cos \phi}{\alpha} \left\{ 1 - \frac{1}{2} \frac{k^2\phi'^2}{\alpha^2} + \frac{1}{2} \cdot \frac{3}{4} \frac{k^4\phi'^4}{\alpha^4} - \&c. \right\}$$

Now

$$\int V d\phi' = \frac{Aa \cos \phi}{\alpha} \left[\phi' - \frac{1}{2} \cdot \frac{1}{3} \frac{k^2}{\alpha^2} \phi'^3 + \frac{1}{2} \cdot \frac{3}{4} \cdot \frac{1}{5} \frac{k^4}{\alpha^4} \phi'^5 - \&c. \right]$$

$$= \frac{Aa \cos \phi}{\alpha} [S_{\phi'}],$$

where $S_{\phi'}$ = the series in brackets.

[It may be noticed that

$$S_{\phi'} = \frac{1}{k} \log(k\phi + \sqrt{\alpha^2 + k^2\phi^2}) - \frac{1}{k} \log \alpha.]$$

It may be shown that $S_{\phi'}$ is convergent if $k\phi' < \alpha$. Now the maximum value of ϕ' is Θ , and the minimum value of α is $A - a$. Hence the series is convergent for all values of ϕ' and α that occur in the second integration if $k\Theta < A - a$. So long as this is the case we shall arrive, after performing the second integration, at a convergent series.

Substituting the above value of $\int V d\phi'$ in equation (I.), we have

$$\begin{aligned} M = & \int_{-\theta}^{2\varpi-\theta} \frac{Aa \cos \phi}{\alpha} (S_{\theta} - S_{-\phi}) d\phi \\ & + \int_{2\varpi-\theta}^0 \frac{Aa \cos \phi}{\alpha} (S_{2\varpi-\phi} - S_{-\phi}) d\phi \\ & + \int_0^{2\varpi} \frac{Aa \cos \phi}{\alpha} (2_{2\varpi-\phi} - S_0) d\phi. \end{aligned}$$

The sum of the last two of these integrals and the second half of the first is zero ; and hence

$$\begin{aligned} M = & \int_{-\theta}^{2\varpi-\theta} \frac{Aa \cos \phi}{\alpha} S_{\theta} d\phi \\ [\text{or } M = & \int_{-\theta}^{2\varpi-\theta} \frac{Aa \cos \phi}{\alpha} \log \frac{k\Theta + \sqrt{\alpha^2 + k^2\Theta^2}}{\alpha} d\phi]. \end{aligned}$$

If Θ is an integral multiple of ϖ (*i. e.* if $\Theta = n\varpi$), we may express M in a series of powers of $\left(\frac{kn\varpi}{A+a}\right)^2$, the coefficients of which are functions of the quantity $\frac{2\sqrt{Aa}}{A+a}$, and the complete elliptic integrals to that modulus.

For in this case

$$\begin{aligned} M = & \int_{-n\varpi}^{2\varpi-n\varpi} \frac{Aa \cos \phi}{\alpha} S_{\theta} d\phi \\ = & 2 \int_0^{\varpi} \frac{Aa \cos \phi}{\alpha} S_{\theta} d\phi. \end{aligned}$$

The general term in this expression for M is

$$2(-1)^m \frac{1 \cdot 3 \cdot 5 \dots 2m-1}{2 \cdot 4 \cdot 6 \dots 2m} \frac{k^{2m}(n\varpi)^{2m+1}}{2m+1} Aa \int_0^{\varpi} \frac{\cos \phi d\phi}{\alpha^{2m+1}}.$$

Now

$$\begin{aligned} \int_0^{\varpi} \frac{\cos \phi d\phi}{\alpha^{2m+1}} &= \int_0^{\varpi} \frac{\cos \phi d\phi}{(A^2 + a^2 - 2Aa \cos \phi)^{\frac{2m+1}{2}}} \\ &= - \frac{2}{(A+a)^{2m+1}} \int_0^{\frac{\varpi}{2}} \frac{\cos 2\theta d\theta}{(1-c^2 \sin^2 \theta)^{\frac{2m+1}{2}}}, \\ &\quad \text{where } c = \frac{2\sqrt{Aa}}{A+a} \\ &= - \frac{2}{(A+a)^{2m+1}} P_m, \quad \int_0^{\frac{\varpi}{2}} \frac{\cos 2\theta d\theta}{(1-c^2 \sin^2 \theta)^{\frac{2m+1}{2}}} \\ &\quad \text{where } P_m = \int_0^{\frac{\varpi}{2}} \frac{\cos 2\theta d\theta}{(1-c^2 \sin^2 \theta)^{\frac{2m+1}{2}}} \end{aligned}$$

Hence we may write

$$\begin{aligned} M &= -\sum 4(-1)^m \frac{1 \cdot 3 \cdot 5 \dots 2m-1}{2 \cdot 4 \cdot 6 \dots 2m} \frac{k^{2m}}{2m+1} \frac{(n\varpi)^{2m+1}}{(A+a)^{2m+1}} Aa P_m \\ &= -4n\varpi \frac{Aa}{A+a} \sum (-1)^m \frac{1 \cdot 3 \cdot 5 \dots 2m-1}{2 \cdot 4 \cdot 6 \dots 2m} \frac{1}{2m+1} \left(\frac{x}{A+a} \right)^{2m} P_m, \\ &\quad \text{where } x = kn\varpi. \end{aligned}$$

Or, writing down the first few terms of the series,

$$\begin{aligned} M &= -4n\varpi \frac{Aa}{A+a} \left\{ P_0 - \frac{1}{2} \cdot \frac{1}{3} \left(\frac{x}{A+a} \right)^2 P_1 \right. \\ &\quad + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{1}{5} \left(\frac{x}{A+a} \right)^4 P_2 \\ &\quad - \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} \cdot \frac{1}{7} \left(\frac{x}{A+a} \right)^6 P_3 \\ &\quad + \frac{1 \cdot 3 \cdot 5 \cdot 7}{2 \cdot 4 \cdot 6 \cdot 8} \cdot \frac{1}{9} \left(\frac{x}{A+a} \right)^8 P_4 \\ &\quad \left. - \&c. \right\}. \end{aligned}$$

It now remains to indicate how $P_0, P_1, P_2, \&c.$ may be calculated.

Let

$$Q_m = \int_0^{\frac{\varpi}{2}} \frac{d\theta}{(1 - c^2 \sin^2 \theta)^{\frac{2m+1}{2}}}.$$

Then

$$P_m = \left(1 - \frac{2}{c^2} \right) Q_m + \frac{2}{c^2} Q_{m-1}, \quad \dots \quad (II.)$$

and

$$Q_m = Q_{m-1} + \frac{c}{2m-1} \frac{d}{dc} Q_{m-1}. \quad \dots \quad (III.)$$

With the help of these two formulæ, observing that

$$Q_0 = F(c),$$

$$Q_{-1} = E(c),$$

and using the relations

$$\frac{dF(c)}{dc} = \frac{E(c)}{c(1-c^2)} - \frac{F(c)}{c}$$

and

$$\frac{dE(c)}{dc} = \frac{E(c) - F(c)}{c},$$

we may express P_0 , P_1 , P_2 , &c. successively in terms of $F(c)$, $E(c)$, &c. The first three are :—

$$P_0 = \left(1 - \frac{2}{c^2}\right) F + \frac{2}{c^2} E,$$

$$P_1 = \left(1 - \frac{2}{c^2}\right) \frac{E}{1-c^2} + \frac{2}{c^2} F,$$

$$P_2 = \frac{2-c^2}{3c^2(1-c^2)} F - \frac{2(1-c^2+c^4)}{3c^2(1-c^2)^2} E.$$

I have not expressed P_3 , P_4 , &c. in terms of F , E , &c., because in practice it is easier to calculate them numerically in any special case as follows, rather than from their expressions in terms of F , E , &c.

(i.) Calculate

$$Q_1, \quad \frac{dQ_1}{dc}, \quad \frac{d^2Q_1}{dc^2}, \quad \frac{d^3Q_1}{dc^3}, \quad \&c.,$$

or

$$Q_1, \quad \dot{Q}_1, \quad \ddot{Q}_1, \quad \dddot{Q}_1, \quad \&c.$$

These may be successively calculated from the equation

$$(1-c^2)Q_1 = E$$

and its derivatives :—

$$c(1-c^2)\dot{Q}_1 - 2c^2\dot{Q}_1 = E - F,$$

$$c(1-c^2)\ddot{Q}_1 + \dot{Q}_1(1-5c^2) - 3cQ_1 = 0,$$

$$c(1-c^2)\dddot{Q}_1 + 4(1-2c^2)\ddot{Q}_1 - 13c\dot{Q}_1 - 3Q_1 = 0,$$

&c.

(ii.) Calculate Q_2 , Q_3 , Q_4 , &c. from the equations :—

$$Q_2 = Q_1 + \frac{c}{3} \dot{Q}_1,$$

$$Q_3 = Q_2 + \frac{c}{5} \dot{Q}_2 = Q_2 + \frac{c}{5} \left\{ \frac{4\dot{Q}_1}{3} + \frac{c}{3} \ddot{Q}_1 \right\},$$

$$Q_4 = Q_3 + \frac{c}{7} \dot{Q}_3 = Q_3 + \frac{c}{7} \left\{ \frac{24}{15} \dot{Q}_1 + \frac{11}{15} c\ddot{Q}_1 + \frac{1}{15} c^2\dddot{Q}_1 \right\},$$

&c.,

which are obtained by successive applications of formula (III.).

(iii.) Calculate P_2 , P_3 , P_4 , &c. from these values of Q_2 , Q_3 , Q_4 , &c. by successive applications of formula (II.).

I have performed the calculations for a circle of 5 inches radius situated in the mean plane of a coaxial helix of 10 inches radius and axial length 4 inches.

In this case,

$$\begin{aligned} A &= 10, \\ a &= 5, \\ x &= 2, \\ c &= \cdot 942809, \\ Q_{-1} &= E = 1\cdot 113741, \\ Q_0 &= F = 2\cdot 528625, \\ Q_1 &= 10\cdot 02366, \end{aligned}$$

$$\begin{aligned} \dot{Q}_1 &= 156\cdot 6023, \\ \ddot{Q}_1 &= 5419\cdot 795 \\ \ddot{Q}_1 &= 156574\cdot 9 \end{aligned}$$

$$\begin{aligned} Q_2 &= 59\cdot 23901 \\ Q_3 &= 419\cdot 7842 \\ Q_4 &= 2207\cdot 927 \end{aligned}$$

$$\begin{aligned} P_0 &= \cdot 54870 \\ P_1 &= 6\cdot 84018 \\ P_2 &= 51\cdot 4955 \\ P_3 &= 391\cdot 442 \\ P_4 &= 1613\cdot 68 \end{aligned}$$

Hence

$$\begin{aligned} M &= n \{ 54\cdot 86225 \\ &\quad - 1\cdot 69790 \\ &\quad + \cdot 10226 \\ &\quad - \cdot 00823 \\ &\quad + \cdot 00041 \} \\ &= n \times 53\cdot 25879 \end{aligned}$$

The above investigation was undertaken in connexion with a measurement of a resistance in absolute measure by the method of Lorenz, which I propose making in the Physical Laboratory at Cardiff. The standard coil which I have constructed for this purpose consists of a helix of copper wire, of

diameter $\cdot 02$ inch, wound in a screw-thread of pitch $\cdot 025$ inch, cut in our Whitworth lathe on a brass cylinder of diameter 22 inches, the number of turns of wire being 185. For such a coil it seemed probable that Lord Rayleigh's formula, which is a first approximation, might not give a result of sufficient accuracy for the purpose in view; and when Lord Rayleigh, with whom I had the privilege of speaking on the matter last summer, expressed this opinion, it became clear that a formula embodying a closer approximation would have to be worked out. This might be done by considering the helix as a current-sheet, and proceeding to a second approximation on this hypothesis. But it seemed to me preferable to make a direct attempt at integration for the case of helix and circle, though I hardly anticipated that I should arrive in the result at a formula of such simplicity as that given above.

It is interesting to observe that, by Lord Rayleigh's formula, the coefficient of mutual induction of the circle and helix taken above for purposes of calculation comes out to be

$$n \times 53\cdot 317.$$

The difference between the two results is about one tenth per cent.

University College of South Wales
and Monmouthshire, Cardiff,
November 8, 1888.

VII. *On the Measurement of the Luminosity and Intensity of Light reflected from Coloured Surfaces.* By Capt. W. DE W. ABNEY, C.B., R.E., F.R.S.*

IN a communication† to the Royal Society General Festing and myself have shown how to compare the light of the different parts of the spectrum reflected from a white surface with that reflected from a coloured surface, and we gave the results of measurements of various colours, and from these constructed their luminosity curves by means of the luminosity curve of the spectrum of white light, which we had ascertained from our previous researches on Colour Photometry (Bakerian Lecture 1886). From the areas of these curves we deduced the total luminosity of these colours, compared with that of a white surface. Certain colours were combined by means of rotating sectors to form a grey, and this was matched with the grey formed by rotating sectors of black and white. By noting the angular value of each

* Communicated by the Physical Society: read November 24, 1888.

† "Colour Photometry," Part II., Phil. Trans. Part A, 1888.

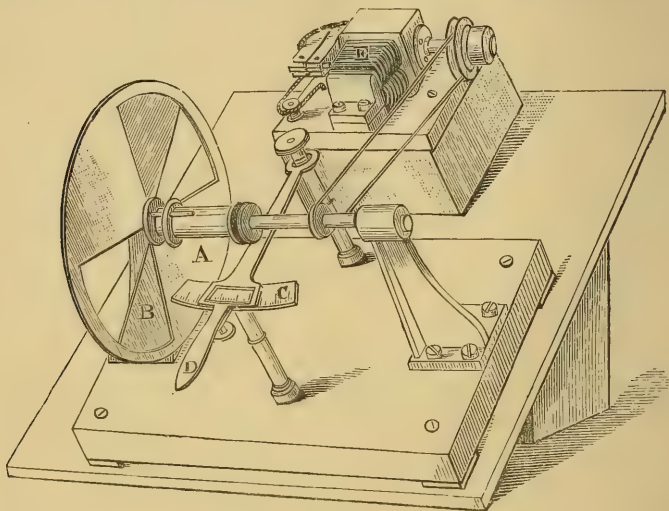
coloured sector, and of the black and white sectors, it was found that the luminosity curves of the colours were correct.

This is an indirect, though very accurate, method of ascertaining the luminosity of a coloured surface, and requires more time than can often be spared with that object in view. Professor O. Rood, in his "*Modern Chromatics*," indicates the manner in which he proceeds, which is as follows. He finds "that with the aid of rotating disks the second constant of colour (luminosity) can often be determined. Let us suppose we wish to determine the luminosity of paper painted with vermilion: a circular disk about six inches in diameter is cut from the paper and placed on a rotation apparatus . . . On the same axis is fastened a double disk of black and of white paper, so arranged that the proportions of black and white can be varied at will. When the whole is set in rapid rotation the colour of the vermilion paper will of course not be altered, but the black and white will blend into a grey. This grey can be altered in its brightness till it seems about as luminous as the red." He then proceeds to give examples. This method must not only be difficult to manage, but also must be tedious before the match can be determined. A method based on the same plan is given later in this paper, which makes the matching of the luminosities more easy.

The following plan, however, I venture to think is much simpler and more certain in result, and is essentially founded on the method which General Festing and myself adopted in measuring the luminosity of the spectrum itself. In that we found that any coloured light might be compared with any other or with white light by *rapidly* changing the luminosity of one colour, when the two were in juxtaposition, making it first decidedly too light and then too dark, and then gradually diminishing the oscillations until an equality of luminosity was obtained. The two colours were placed alongside one another, it may be recollected, by means of the Rumford method of shadows. In the case of the spectrum the rapid diminution in luminosity from the yellow to each end of the spectrum enabled the change of luminosity to be quickly made by sliding the card containing the slit (which allowed a slice of coloured light to pass and to subsequently form a patch of that colour on the screen) along the spectrum on each side of the maximum, and then noting the position of the colour, which balanced a white light of known intensity. In the case of the measurement of the luminosity of coloured paper, this method evidently was inapplicable, and it remained to devise some other.

In our former experiments of the measurement of light

reflected from coloured surfaces, General Festing and myself employed rotating sectors, movable when in motion, but the motion was slow, being effected by means of a screw. Mr. Hilger, in whose hands was placed a rough design for an improved set of rotating sectors, produced an instrument in which the sectors would be opened and closed rapidly by means of a simple lever arrangement without any screw-motion. The instrument is shown in the accompanying figure. A, B are the sectors which can be opened at pleasure by means of the lever, D, moving along an arc, C, on which is a scale of degrees. The sectors are rotated by a small electromotor, with sufficient speed to prevent scintillation.



The apparatus at once got over the difficulty of getting a



rapid alternation of intensity in any light falling on a surface when the instrument was placed between it and such surface.

Supposing the luminosity of a vermilion-coloured surface had to be compared with a white surface when both were illuminated, say, by gas-light, the following procedure was adopted:—A space of such a size was cut out of black paper that one side was rather less than twice the breadth of the rod used to cast a shadow. One half of the aperture was filled with a white surface and the other half with the vermilion-coloured surface. The light L was caused to illuminate the whole of this, and the rod R placed in such a position that it cast a shadow on the white surface, the edge of the shadow being placed accurately at the junction of the vermilion and white surfaces. A flat unsilvered mirror M was placed at such a distance and at such an angle that the light it reflected cast a second shadow on the vermilion surface. Between R and L were placed the rotating sectors A B. The white strip was then caused to be evidently too dark and then too light by altering



the aperture of the sectors, and an oscillation of diminishing extent was rapidly made till the two shadows appeared equally luminous. A white screen was substituted for the vermilion, and again a comparison made. The mean of the two sets of readings of angular apertures gave the relative value of the two luminosities. It must be stated, however, that if the screen remained open, as represented, the values would not be correct, since any diffused light which might be in the room would relatively illuminate the white surface more than the coloured one. To obviate this the receiving screen was placed in a box in the front of which a narrow aperture was cut just wide enough to allow the two beams to reach the screen. An aperture was also cut at the front angle of the box through which the observer could see the screen. When this apparatus was adopted its efficiency was seen from the fact that when the apertures of the rotating sectors were closed the shadow on the white surface appeared quite black, which it would not have done had there been any diffused light in any quantity present within the box. The box, it may be stated, was blackened inside and was used in a darkened

chamber. The mirror arrangement was useful, as any variation in the direct light also showed itself in the reflected light. Instead of gas-light, reflected sky-light, the electric light, or sunlight can be employed by very obvious artifices; for it must be remembered that the comparison-light may be of any kind, and distinct, if necessary, from the light illuminating the coloured surface.

General Festing and myself had measured with great care the luminosities of emerald green, vermilion, and ultramarine, which we had combined in a rotation apparatus to produce a grey. The total luminosity of these three colours I measured by the apparatus described, using as the source of light to illuminate the colour, the patch of white light formed by a recombination of the spectrum, the comparison light being an ordinary gas-light.

The following table gives the values obtained by the spectrum method and also by the method I now introduce. It will be seen that the coincidence between the two values is very close:—

Colour.	Area of Luminosity Curves (from Paper in the Phil. Trans.).	Luminosity Readings by New Method.		Value from areas when white = 100.	Value from New Plan when white = 100.
		Readings.	Mean.		
White	534	73 77, 75, 76	75	100	100
Emerald Green .	221	32, 28, 29, 31, 31	30·2	41·1	40·3
Vermilion	216	33, 30, 29, 29	30·2	40·0	40·3
French Ultramarine.....	49	6, 7, 7, 7·5	6·9	9·2	9·2

For another purpose I had measured the luminosity curves of six different colours by the spectrum method, and below is a table of the intensities of the light of different wave-lengths reflected from each and from a white surface. The areas indicate the total luminosities of the several colours.

Intensity Curves.

Scale No.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.	White.
43	60	80	75	0	6	37	100
44	74	88	80	4	14	41	100
45	90	90	90	7	22	43	100
46	96	84	80	12	28	43	100
47	94	70	40	16	34	40	100
48	90	50	16	21	40	39	100
49	80	40	10	26	44	39	100
50	66	30	6	32	46	40	100
51	52	22	5	41	40	40	100
52	40	17	4	51	32	40	100
53	38	13	4	56	24	38	100
54	35	10	3	59	17	35	100
55	32	6	3	60	13	32	100
56	31	3	2	57	10	29	100
57	31	2	2	53	8.5	27	100
58	30	1	2	50	8	25	100

From this table are deduced the luminosity curves which are in the following table, the luminosity curve of the white being that given in the Bakerian lecture for 1886.

Luminosity Curves.

Scale No.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.	White.
43	1.5	2.0	1.8	0	1.2	.9	2.5
44	12.2	16.5	13.2	.6	2.3	6.7	16.5
45	58.5	58.5	58.5	4.5	14.3	28.0	65.0
46	90.2	79.0	75.2	11.2	26.3	40.4	94.0
47	94.0	70.0	40.0	16.0	34.0	40.0	100.0
48	84.6	47.0	15.0	19.7	37.6	36.7	94.0
49	64	32.0	8.0	20.8	35.2	31.2	80.0
50	28.7	13.0	2.6	13.9	20.2	17.4	43.5
51	10.0	4.4	.9	8.0	7.7	7.8	19.4
52	3.8	1.8	.4	4.9	3.0	3.8	9.6
53	2.0	.7	.3	3.2	1.4	2.2	5.8
54	1.2	.4	.2	2.1	.3	1.3	3.6
55	.7	.1	.1	1.2	.2	.7	2.1
56	.47	.1	.4	1.3
57	.35	.1	.3	1.0
58	.232	.6
Areas...	453	323	217	107	183	218	534

[The scale-numbers which apply to the Fraunhofer-lines are as follows :—A = 42·56, B = 43·34, C = 44·26, D = 46·54, E = 49·62, F = 52·08, G = 57·72.]

The areas of the curves are those shown in the table. Taking the area of the luminosity curve of white as 100, the following are the luminosities of the colours derived from the above :—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
100	84	60	41	20	34	41

The readings by the new method were :—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
77	65	45	31	15·5	27	31

Taking white as 100, as before, the readings reduce to :—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
100	84	58	40	20	35	40

The exactitude of the latest method is again exemplified, but it is not pretended that it is quite so accurate as the older method.

The method described is particularly adapted to a coloured surface of small dimensions. Where, however, a circle of some 3 or 4 inches in diameter can be utilized, the following modification of Rood's method may be adopted. Behind the sectors and on the spindle of the rotating disks, a disk of white card can be placed. The sectors being blackened on the other side, when revolved we can produce any shade of grey, from half black and half white to all black. On the other side of the sectors can be placed a smaller disk of coloured card, to be measured, and this will rotate with the sectors. Should the colour be a fairly dull one, the match of luminosity can be made by altering the shade of grey by rapid oscillation in aperture of the sectors, and the luminosity of the colour in terms of white determined when the amount of white light reflected from the black surface has been measured. Should the colour be more luminous than the grey given by half black and half white, the coloured disk can be toned down by means of a black semi-disk, adding a known quantity of the black surface by Maxwell's plan. The luminosities of the surfaces are then measured, and the true luminosity of the coloured surface in terms of the white surface, calculated after allowing for the added black.

This same plan may be adopted for measuring the *intensity* of the light of the various parts of the spectrum reflected from a coloured surface, in comparison with that reflected from a white surface. Colour patches of monochromatic light are

formed by the method given in the *Philosophical Magazine* for 1885 (vol. xx.), which fall on the coloured disk and the rotating sectors, which form a grey in white light. There is less difficulty in this case than in ascertaining the luminosity, as the colour falling on the mixture of black and white is the same as that falling on the pigment. In this case, too, it may be necessary to add a fixed amount of black to the coloured disk in order to get a reading.

VIII. *Note on a Modification of the Ordinary Method of Determining Electromagnetic Capacity.* By J. W. W. WAGHORN, D.Sc.*

THE object of the proposed modification of the ordinary well-known method is to enable the capacity of a condenser or other conductor to be determined with sufficient accuracy without the costly apparatus generally employed.

The usual process consists, as is well known, in reading the "throw," d_1 , of a galvanometer-needle under the impulse due to the flow of a quantity of electricity which charges the condenser to the potential of a certain battery, and in afterwards reading the steady deflexion, d_2 (not very different from d_1), when the galvanometer is connected in series through a total resistance R .

$$\text{Then the required capacity, } F \text{ (in farads)} = \frac{t \times d_1}{2\pi R d_2};$$

where t is the time of one complete vibration of the needle.

But as "damping" reduces the throw of the needle, the logarithmic decrement has to be determined and allowance made accordingly.

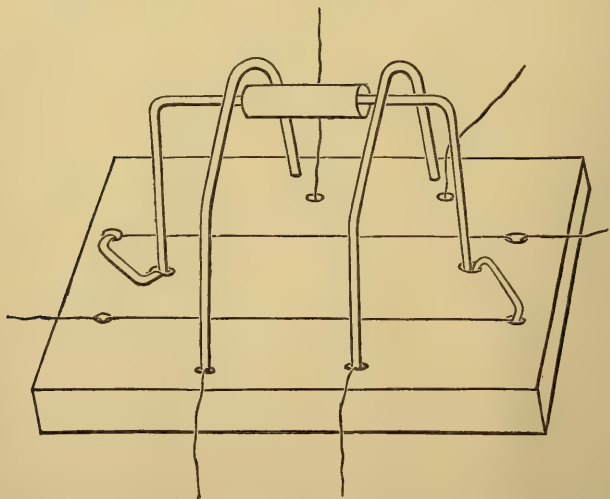
In attempting this determination of capacity with a sensitive galvanometer not specially designed for ballistic observations, the difficulties occur that in the first place the resistance, R , must be very great if d_1 is not to be very different from d_2 ; if the capacity of the condenser is $\frac{1}{2}$ microfarad, for example, and the time of vibration two seconds, the resistance required is more than 600,000 ohms, and these large resistances are not always available. Secondly, by reason of the shortness of the fibre suspension and its imperfect elasticity, the needle does not immediately take up its true position under the forces acting upon it, an imperfection which causes the deflexion due to the throw of the needle to be unfairly lessened in comparison with the steady deflexion against which it is compared;

* Communicated by the Physical Society: read December 8, 1888.

the final result being that in ordinary cases the capacity of the condenser may work out one, two, or even four per cent. less than its true value.

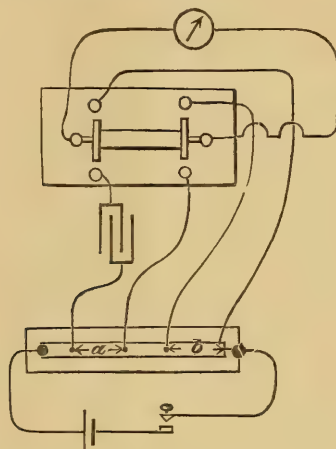
The first difficulty, the use of very high resistances, can be avoided, as suggested by Prof. Ayrton, by reducing the E.M.F. of the battery in a known ratio when obtaining the steady deflexion ; but the second seems more troublesome to avoid, unless we are supplied generally with Mr. Vernon Boys's quartz fibres ; but the difficulty may be turned if, instead of reading the permanent deflexion of the galvanometer, the throw on completing the circuit is read. This throw would, of course, if there were no damping, be double the permanent deflexion, and the damping need not be allowed for, since it is (at least very approximately) the same in the two observations made ; that is, in the throw due to charging the condenser and also in the first elongation of the needle due to the alteration in the permanent field.

The requisite apparatus becomes then very simple, and such as is always likely to be at hand whenever such observations are required, and involves merely resistance-coils of, say, from 1 to 10,000 ohms with wandering leads, a sensitive galvanometer, and battery. A Pohl's commutator or rocking key is very convenient, but of course not indispensable, for comparing in rapid succession the two throws which form the actual experiment.



The ordinary form of this commutator slightly modified, as shown, is very convenient in all the numerous electrical observations which require a comparison between any two

deflexions, either steady or momentary, of an instrument such as a galvanometer or electrometer.



Let α be the resistance in that part of the resistance-coils between which the condenser circuit is applied, b the resistance in that part of the coils between which the permanent current is applied. The galvanometer itself forms a sufficient resistance to insert in the permanent-circuit branch, but its resistance must be known at the temperature at which it is used.

$$F \text{ now} = \frac{t \times d_1 \times b}{\pi \times G \times d_2 \times a}.$$

The above expression is not strictly accurate, because damping occurs on an open circuit when the condenser is being charged, and on a closed circuit when the permanent current is flowing ; but in a galvanometer with any approach to ballistic properties the error may be considered as negligible for ordinary purposes ; for example, the logarithmic decrement of a galvanometer of 2000 ohms used in this operation on closed circuit was .0428, on open circuit .0415, involving an error of only one part in 2000, if the damping is entirely neglected in the two cases. With a Thomson galvanometer, in which the decrement reached the value of .56 on open circuit, the error involved would, however, amount to one per cent.

The imperfect elasticity of the fibre may still exert a small influence, because the needle is swinging for a quarter period in the condenser charge and for a half period in the permanent-current observation ; but if the error is not entirely obviated, it is brought down to very small limits.

Any sensitive galvanometer whose time of vibration exceeded two seconds, and in which there was not excessive damping, would probably be suitable for this determination; if the period is too small, among other difficulties, that of self-induction, delaying the passage of the permanent current, would occur.

With the apparatus connected as shown to a galvanometer whose resistance is known, the actual determination need not take more than two or three minutes, and involves but very little more trouble than is necessary to determine the capacity in terms of a standard condenser, without reposing the confidence which is required in that case. With a galvanometer not specially chosen for the purpose, the capacity of a half microfarad condenser, tested on several different occasions by different observers, gave $\cdot 497$ for the lowest value and $\cdot 501$ for the highest; but tested by the ordinary steady-deflexion method, the value determined was only $\cdot 485$, the error in this case being due entirely to the imperfect elasticity of the short fibre suspension.

The plan is not, however, suggested as a means of standardizing condensers where more refined means are available, but in the numerous cases where long-fibre ballistic galvanometers are not at hand and where an accuracy of, say, one half or one quarter per cent. is deemed sufficient.

IX. *On Thermoelectric Phenomena.* By J. PARKER, B.A.,
late Scholar of St. John's College, Cambridge*.

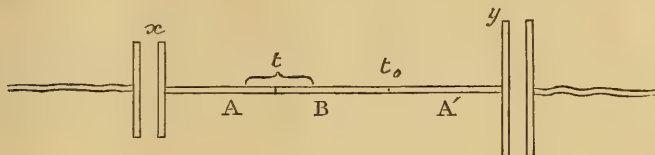
THE present short paper is necessary to complete the paper which I published on the same subject in the *Philosophical Magazine* in October 1888.

In that paper, a constant k was discovered which was tacitly supposed to have different values for different metals. Since then, however, owing to a suggestion made by Prof. Liveing, I have been led to reconsider the subject, and I now find that the constant k must have the same value for all metals. This remarkable discovery introduces some important simplifications.

Let two condenser-plates, x, y , of any the same metal be joined by three wires, A, B, A', as in the figure; A, A' being of the same metal as x and y , and B being different. Let the plates (x, y) and the junction of the wires (B, A') be kept at the same absolute temperature t_0 , and the junction of the wires (A, B) at the absolute temperature t , where $t - t_0$ is indefinitely small, and $= \tau$, say.

* Communicated by the Author.

Parallel and close to the plates (x, y) place equal plates of any metal whatever, as iron, and connect these iron plates by long iron wires with a large distant mass of iron in the neutral state.



By slowly separating the plates x , and slowly bringing the plates y nearer together, let unit charge be made to pass from x to y , without altering the potential of either x or y . By equation (8), the heat absorbed at the junction of the hot and cold parts of the wire A will be

$$\left(k_{\alpha}t + \frac{df_{\alpha}}{dt}\right)\tau;$$

and at the junction of the hot and cold parts of the metal B,

$$-\left(k_{\beta}t + \frac{df_{\beta}}{dt}\right)\tau.$$

Now let the wires A, B, A' be removed; secondly, reduce the potential of the plate y to equality with that of x ; thirdly, make the unit charge return from y to x in a reversible manner; finally, bring the potential of y back to its original value. All this may be accomplished without producing any thermal effect. A complete reversible cycle of operations having been performed, we have, by Carnot's principle,

$$\tau\left[\left(k_{\alpha} + \frac{1}{t} \frac{df_{\alpha}}{dt}\right) - \left(k_{\beta} + \frac{1}{t} \frac{df_{\beta}}{dt}\right) + \frac{d}{dt}\left(\frac{\Pi}{t}\right)\right] = 0. \quad (15)$$

But we have already seen that

$$\frac{d}{dt}\left(\frac{\Pi}{t}\right) = \frac{1}{t} \frac{d}{dt}(f_{\beta} - f_{\alpha}). \quad \dots \dots (3)$$

Hence (15) becomes

$$k_{\alpha} = k_{\beta}. \quad \dots \dots (16)$$

Thus, if two portions of the same metal, at different absolute temperatures t, t_0 respectively, be in contact, and if V, V_0 be the potentials which they assume, then

$$V - V_0 = \frac{1}{2}k(t^2 - t_0^2); \quad \dots \dots (7)$$

where k is a constant which has the same value for all metals. In other words, the difference of potential depends only on the temperatures, and is the same for all metals. If, however, unit charge be made to cross the junction, the heat that must

be imparted to the junction to keep its temperature constant will be different for different metals.

From the important equation (7) it follows that, supposing the constant k not to be zero, when contact is made between two pieces of the same metal at different temperatures, there will be a temporary current across the junction which is in the same direction for all metals. From this rule those metals are excepted which, on being heated, undergo some permanent molecular change, so that their state is not completely defined by the pressure and temperature.

When a thermoelectric circuit is formed of two metals, equation (9) for the electromotive force takes the simple form

$$E = \delta - \delta_0, \quad . \quad . \quad . \quad . \quad . \quad (17)$$

which gives at once the well-known equation

$$\Pi = t \frac{dE}{dt}. \quad . \quad . \quad . \quad . \quad . \quad (18)$$

When the temperatures of the two junctions differ by an infinitesimal quantity τ , we have, instead of (10), the following form for the electromotive force :—

$$E = \frac{\Pi}{t} \cdot \tau, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

a result due to Sir W. Thomson.

In general, if a thermoelectric circuit be formed of any number of different metals, and if δ be the abrupt rise of potential at any junction as we travel in the direction of the current, then evidently $E = \Sigma \delta. \quad . \quad . \quad . \quad . \quad . \quad (20)$

If the junctions be all at the same temperature, we have $\Sigma \delta = 0$, and therefore $E = 0$.

I wish to add, in conclusion, that I have lately learned that the subject of thermoelectricity has been already considered by Duhem (*Annal. de Chim.* Dec. 1887), and that the results at which he has arrived are in complete accord with those which I have obtained in my two papers on the subject.

X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxvi. p. 314.]

November 7, 1888.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. "The Permian Rocks of the Leicestershire Coal-field." By Horace T. Brown, Esq., F.G.S.

The author considers that whilst rocks belonging to the Car-

boniferous and Trias have been mapped as Permian, true representatives of the Permian do exist in the district to a considerable extent. The Bunter conglomerates rest for the most part upon the truncated edges of Carboniferous strata; but intercalated between them and the Carboniferous, at various points, are thin beds of purple marly breccias and sandstones seldom exceeding from 30 to 40 ft., but differing in lithological character from the overlying and underlying rocks. The brecciated series rests with striking unconformity upon the Carboniferous. Moreover, the Boothorpe fault, which throws the Coal-measures 1000 ft., affects the overlying brecciated series to an extent of not more than from 20 to 30 ft. The unconformity between the brecciated series and the Bunter is less obvious. Sections establishing the double unconformity were described in considerable detail. Attention was also called to other localities within the Coal-field where Permian rocks exist, the author having in many cases mapped their boundaries.

He further called attention to certain beds which have been erroneously classed as Permian by the Survey. The first of these is a patch at Knowle Hills. Making extensive use of the hand-borer, he found that the greater part of the so-called Permian consists of a wedge-shaped piece of Lower Keuper let down by a trough fault. The so-called Moira grits belong to and are conformable with the ordinary Coal-measures of the district.

The lithological characters of the Leicestershire Permians are sufficient to differentiate them from the Trias and Carboniferous. They consist of red and variegated marls, bands of breccia, and beds of fine-grained yellowish sandstone; the breccia fragments are of great variety and little waterworn. These are imbedded in a bluish-grey matrix, hard or soft, which consists of insoluble matter united by the carbonates of lime and magnesia with some hydrated ferrous oxide, which on exposure becomes oxidized.

The breccias have a tendency to die out northwards. The most abundant materials are quartzo-felspathic grits with associated grey flinty slates (Older Palæozoic), with in addition vein-quartz, volcanic ash, and igneous rocks. The Carboniferous rocks afford argillaceous limestone, Mountain Limestone, grits, and hæmatite. At Boothorpe nearly 90 per cent. is made up of the old Palæozoic material, whilst at Newhall Park 28·8 per cent. consists of Carboniferous grits and hæmatite. The quartzite fragments resemble those of the lower part of the Hartshill series, but the existence of "strain shadows" indicates a difference subsequently explained. A very few fragments may be referred to the Charnwood rocks.

The bulk of the material has a southern origin, and the irregularity of the fragments proves that they cannot have come from a distance. Evidence is given of the probable existence of a ridge of older Palæozoics, from which the Carboniferous rocks had been stripped, beneath the Trias of Bosworth. (There is an actual outcrop of Stockingford shales at Elmesthorpe.) The direction of this line is parallel with the Nuneaton-Hartshill and Charnwood axes of

elevation, and also with the general direction of the major folds and faults of the Leicestershire Coal-field. The northern part of this ridge, which is apparently a faulted anticlinal, is a very probable source of the angular fragments occurring in the Permian breccias 5 or 6 miles to the north-west.

The author concluded that the Permian rocks of the Leicestershire Coal-field belong to the same area of deposition as those of Warwickshire and South Staffordshire, all having formed part of the detrital deposits of the Permian Lake which extended northwards from Warwickshire and Worcestershire, and which had the Pennine chain on its eastern margin. He pointed out the dissimilar nature of these deposits to those of the eastern side of the Pennine chain from Nottingham to the coast of Durham. There were proofs of the existence of a land barrier, owing to the uprising of the Carboniferous, between the district round Nottingham and the Leicestershire Coal-field. The most northerly exposure of the Leicestershire Permians is 13 miles S.W. of those of South Notts. He indicated the probable course of the old coast-line of the western Permian Lake. Denudation had bared some of the older Palæozoics of their overlying Coal-measures, and it is the rearranged talus from the harder portions of these older rocks which now form the brecciated bands in the Leicestershire Permians.

In an Appendix some igneous rocks found in the Bosworth borings were described.

2. "On the Superficial Geology of the Central Plateau of North-western Canada." By J. B. Tyrrell, Esq., B.A., F.G.S., Field Geologist of the Geological and Natural History Survey of Canada.

The Drift-covered prairie extends from the west side of the Lake of the Woods to the region at the foot of the Rocky Mountains, rising from a height of 800 feet on the east to 4500 feet on the west, the gentle slope being broken by two sharp inclines known as the Pembina Escarpment and the Missouri Coteau, giving rise to the First, Second, and Third Prairie Steppes.

The author described the older rocks of this region, referring especially to his subdivision of the Laramie Formation into an Edmonton Series of Cretaceous age, and a Pascapoo Series forming the base of the Eocene, and then discussed the Superficial Deposits in the following order:—

1. *Preglacial gravels* occurring along the foot of the Rocky Mountains, composed of waterworn quartzite pebbles, similar to those now forming and, like them, produced by streams flowing from the mountains.

2. *Boulder-clay* or *Till*, having an average thickness of 50–100 feet, and filling up preexisting inequalities. The clay is essentially derived from the material of the underlying rocks. The smoothed and striated boulders of the western region are largely quartzites derived from the Rocky Mountains; these gradually disappear towards the east, and are replaced by gneisses and other rocks transported from the east and north-west. Towards the north-west several driftless hills over 4000 feet high appear to have stood

as islands above the sheet of ice. Some of the surface erratics of gneissose rock have doubtless been derived from the Till, whilst others are connected with moraine deposits, and others, again, appear to have been dropped from bergs floating in seas along the ice-front. The Till is sometimes divisible into a lower massive and upper rather stratified deposit, separated occasionally by

3. *Interglacial Deposits* of stratified material, with seams of impure lignite, and shells of *Pisidium*, *Limnæa*, *Planorbis*, &c.

4. *Moraines*, which are intimately associated with the Boulder-clay, and represent terminal moraines of ancient glaciers which originated upon or crossed the Archæan belt. One of these is the well-known Missourie Coteau.

After pointing out the derivation of quartzite pebbles in the drifts of the eastern region from Miocene conglomerates, and not directly from the Rocky Mountains, the author described

5. The *Kames* or *Asar* generally occurring at the bottoms of wide valleys, and which resemble in structure those of Scandinavia.

6. *Stratified Deposits* and Beach-ridges which have been formed at the bottoms and along the margins of freshwater-lakes lying along the foot of the ice-sheet. The principal of these occupied the valley of the Red River, and has been called Lake Agassiz; it had a length of 600 miles and a width of 170 miles. The author described in detail the gravel terraces formed around this lake, and showed that a slow elevation had taken place towards the north and east since their formation. He favoured the view that the waters of the lake were dammed by the ice towards the north. An account was given of some quartzite flakes, apparently chipped by human agency, in one of the terraces of this lake. On the recession of the ice the southern drainage-channel was abandoned, and a northerly one opened out.

7. *Old Drainage-channels*.—Throughout the whole region old drainage-channels appear to have been occupied by southerly running rivers (where the present drainage is northerly), and are considered to have carried away the waters draining from the foot of the ice. Some of these valleys have been blocked by moraines in the Duck Mountains, the result of local glaciers.

Nov. 21.—W. T. Blanford, LL.D., F.R.S., President, in the Chair.

W. Whittaker, Esq., B.A., F.R.S., F.G.S., who exhibited a series of specimens from the deep boring at Streatham, made some remarks upon the results obtained, of which the following is an abstract:—

After passing through 10 feet of gravel &c., 153 of London Clay, 88½ of Lower London Tertiaries, 623 of Chalk (the least thickness in any of the deep borings in and near London), 28½ of Upper Greensand, and 188½ of Gault, at the depth of 1081½ feet hard limestone, mostly with rather large oolitic grains, was met with. This, with alternations of a finer character, sandy and clayey, lasted for only 38½ feet, being much less than the thickness of the Jurassic beds, either at Richmond or at Meux's boring. The general character of the cores showed a likeness to the Forest Marble, and the occurrence of *Ostrea acuminata* agreed therewith.

At the depth of 1120 feet the tools entered a set of beds of much the same character as those that had been found beneath Jurassic beds at Richmond, and beneath Gault at Kentish Town and at Crossness. The softer and more clayey components were not brought up; the harder consist of fine-grained compact sandstones, greenish-grey, sometimes with purplish mottlings or bandings, and here and there wholly of a dull reddish tint. With these there occur hard, clayey, and somewhat sandy beds, which are not calcareous, whilst most of the sandstones are. Thin veins of calcite are sometimes to be seen, and at others small concretionary calcareous nodules; but no trace of a fossil has been found.

The bedding is shown, both by the bands of colour, and by the tendency of the stone to fracture, to vary generally from about 20° to 30°.

In the absence of evidence it is hard to say what these beds are, and the possibilities of their age seem to range from Trias to Devonian. It is to be hoped that this question may be solved, as on it depends that of the possibility of the presence of Coal-measures in the district; and Messrs. Docwra, the contractors of the works, have with great liberality undertaken to continue the boring-operations at their own expense for at least another week.

Details of the section will be given in a forthcoming Geological Survey Memoir, in which, moreover, the subject of the old rocks under London will be treated somewhat fully.

The following communications were read:—

1. "Notes on the Remains and Affinities of five Genera of Mesozoic Reptiles." By R. Lydekker, Esq., B.A., F.G.S.

2. "Notes on the Radiolaria of the London Clay." By W. H. Shrubsole, Esq., F.G.S.

3. "Description of a new Species of *Clupea* (*C. vectensis*) from Oligocene Strata in the Isle of Wight." By E. T. Newton, Esq., F.G.S.

XI. Intelligence and Miscellaneous Articles.

ON THE EXCITATION OF ELECTRICITY BY THE CONTACT OF RAREFIED GASES WITH GALVANIC INCANDESCENT WIRES. BY J. ELSTER AND H. GEITEL*.

IN a series of experimental investigations published in Wiedemann's *Annalen*, the authors had treated the case of the excitation of electricity on the contact of gases and ignited bodies for the case in which the gases were either under the pressure of the atmosphere or under a pressure of at least 10 millim. mercury. In the present investigation it is attempted to establish the phenomenon for very minute pressures.

It results that, in accordance with previous results, using galvanic incandescent platinum wires of over 0.2 millim. in thickness,

* An experimental investigation made at the expense of the Elizabeth Thompson Science Fund, Boston, U.S.A.

oxygen is *positive* even at the highest rarefaction, but hydrogen is *negative*. Air, aqueous vapour, sulphur, and phosphorus vapour appear, when pure, to be excited positively, but more feebly than oxygen; mercury, on the contrary, appears to be indifferent. The products of decomposition of fatty vapours, which, from using greased stopcocks and connexions, penetrate into the receiver of the mercury-pump, acquire stronger negative charges than hydrogen. By continued action of the ignited wires the deportment of the rarefied gases, as well as of the wires, varies, especially if they are thinner than 0.2 millim.; their molecular structure is altered by continuous ignition.

The nature of the electrodes is of small influence; in like manner the occlusion of gases by wires, as well as the detachment of solid particles during ignition, appears to play no essential part. Only by using ignited *carbon threads* does the disengagement of the gases absorbed by the carbon come into play; incandescent carbon-filaments in a glow-lamp always electrify *negatively* the residues in the surrounding medium.

Incandescent platinum-iridium, palladium, and iron wire do not differ essentially from platinum wire.

The electromotive force is further dependent on the action of extraneous magnetic forces. In rarefied hydrogen it is influenced by the occurrence of Hall's phenomenon; the existence of which could not be shown with the same certainty in oxygen. In the latter gas the magnet produces in general an increase of the positive, in hydrogen, acting in the same direction, a decrease of the negative, electrifications up to the change of sign; at the same time the position of the poles exerts some influence.

The phenomena of unipolar conduction display themselves in rarefied gases like those of normal density; that electricity is always more easily discharged whose sign is opposed to that excited by the process of ignition in the gas. In the magnetic field the conductivity of the gas in contact with the ignited wire increases for negative electricity; here also the position of the poles has a determining influence.

The authors attempt to bring the results of their investigation in agreement with Schuster's theory of the electrolytic conductivity of gases, and under the influence of a dissociation of the gaseous molecules at the ignited body.

In conclusion the authors point to the connexion of their investigations with those of Goldstein and Hittorf on the passage of electricity through rarefied gases using ignited electrodes, and ascribe the results of the experimenters in question to the electromotive force which occurs at the ignited wire.—*Sitzungsberichte der Wiener Akademie*, October 23, 1888.

ON THE PHYSICAL PROPERTIES OF THIN SOLID LAMINÆ.

BY G. QUINCKE.

The common boundary of two liquids has the tendency to

become as small as possible. The force α_{12} , which is at work in the common boundary of the liquids 1 and 2, has been called the surface-tension. Eighteen years ago I described several methods of determining it*.

Analogous forces must be assumed to exist at the boundary of a solid with air, or with another liquid. These surfaces also have the tendency. The phenomena at the boundary of a liquid and of a solid are essentially different from those at the boundary of two liquids, for in the former there is no lateral displacement of the particles of the solid.

While the boundary of two liquids forms spheres or spherical shells, if we disregard the action of gravity, the boundary of a liquid and of a solid forms folds, and under certain conditions cylindrical forms, or tubes.

I obtained thin solid laminæ by allowing albumen or aqueous solutions of glue, or alcoholic solutions of resins, to dry on mercury the surface of which had been covered with a trace of fat.

The periphery of the solid lamina forms thus a sine curve, which lies upon a vertical cylinder surface, and is connected with the centre of the lamina by radial straight lines. The periphery of the solid lamina is then alternately higher and lower than the original horizontal surface of mercury.

On the periphery there can be n elevations and n depressions, where n is any whole number 1, 2, 3, 20, 100, or more.

The thinner the lamina and the greater its diameter, the greater in general is n , and the smaller the vertical height of the elevations and depressions.

The edge of the solid lamina may also be periodic in several ways. Thus, for instance, there can be simultaneously two or three great folds, and twenty-four or still more small folds at the periphery.

The magnitude and shape of the solid lamina depends on the surface-tension of the fatty surface of mercury, that is to say, on the thickness of the fatty layer on mercury, on the temperature, and on the radiation, so that heliotropism can be discovered on it.

Solid laminæ, the thickness of which is less than 0.000045 millim., may modify the form of the surface and exhibit a folded surface. The thickness may be so small that it can no longer be perceived with the microscope.

If flat air-bubbles in water or flat drops of mercury in air are coated with very thin solid laminæ, the form of the bubbles and drops are modified in the same way as by a coating with a solid lamina.

Thin solid laminæ of glue, resins, soap, albumen, thin metal layers, formed cylindrical shapes or tubes on the surface of mercury, water, chloroform, or fatty oils, with air or with other liquids, if the surface tended to become as small as possible, and was prevented from assuming the spherical shape by not being able to move laterally.—*Berliner Berichte*, July 12, 1888.

* Pogg. *Ann.* vol. cxxxix. (1870).

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[FIFTH SERIES.]

FEBRUARY 1889.

XII. *On the Polarization of Sky Light.* By JAMES C. MCCONNEL, M.A., Fellow of Clare College, Cambridge*.

[Plate VII.]

THIS “mysterious and beautiful phenomenon,” as it was called by Sir John Herschel, must be now admitted to have been satisfactorily explained. It has been shown by Brücke (*Pogg. Ann.* vol. lxxxviii.), Tyndall (*Proc. Roy. Soc.* Jan. 1869), and others, that an artificial blue sky may be produced in water or air by the introduction of myriads of very minute particles of some denser substance, and that the light scattered by these particles in a direction perpendicular to the incident beam is completely polarized in the plane of incidence. Lord Rayleigh has placed the matter on a more satisfactory basis by deducing the same results from the electromagnetic theory of light†. He further defined accurately the colour of the scattered light, showing that the amount scattered of different kinds of homogeneous light varied inversely as the fourth power of the wave-length. With regard to the intensity and polarization of light scattered in different directions he obtained the following results. To find the nature of the light deflected in any given plane,

* Communicated by the Author.

† He first employed a form of the “elastic solid” theory in which the density of the æther is supposed to vary in different media (*Phil. Mag.* Feb., April, June, 1871). In a later paper (*Phil. Mag.* Aug. 1881) he showed that the electromagnetic theory led to the same equations.

divide the incident ray into two parts, that polarized in the plane of deflexion and that polarized in the perpendicular plane. Of the former the proportion deflected is the same in every direction, but of the latter the proportion deflected varies as the square of the cosine of the angle between the original and the new directions of propagation, being zero when this angle is a right angle. It may be added that the proportion of light thrown straight backwards is independent of the direction of the original polarization, as is indeed obvious from symmetry. For the truth of these laws it is essential that the particles should be small compared with the wave-length of the kind of light involved. I gather that they would be practically accurate for particles which were not larger than a tenth part of the wave-length, while with roundish particles whose diameter reached half a wave-length quite different phenomena would set in.

The experimental verification of these laws in the case of the sky cannot be expected to be very accurate, owing to the complexity of the problem. Still sufficient has been accomplished to render it almost certain that the greater part of the light from a good blue sky has been scattered by small particles, small, that is, compared with a wave-length. Lord Rayleigh has compared the colour of sky light with that of sunlight diffused through white paper, obtaining very fair agreement with the law of the inverse fourth power of the wave-length (*Phil. Mag.* Feb. 1871). Further, Abney has found, on measuring the relative intensities of the various components of sunlight on different days and with different altitudes of the sun, that the very considerable variations could be explained on the assumption that practically the whole of the light lost in traversing the atmosphere had been scattered according to the above law*. This of course was only the case when the atmosphere was quite clear. These observations show not merely that the sky light is due to small particles, but also that the true absorption (other than scattering) of the visible radiation by the atmosphere is very small.

So much for the colour of the sky. With regard to the polarization we find, as we should expect from the theory, the maximum polarization at points in the sky about ninety degrees from the sun. Rubenson, at Upsala, took eighteen measures of the position of the point of maximum polarization in the vertical plane through the sun on different days and at different hours. The distance from the sun varied

* *Phil. Trans.* March 1887. He mentions, however, that Langley's bolometer observations do not support this law.

between $88^{\circ} 23'$ and $92^{\circ} 8'$, the mean being $90^{\circ} 2'$. Of course the polarization even at the maximum point is far from being complete. For light reaches the scattering particles from the sky and the earth as well as the sun. Every ray that impinges on a particle sends a scattered ray to the observer. And these scattered rays are polarized, some partially, some completely, in all sorts of different planes. The net result is a quantity of subsidiary light that dilutes the completely polarized rays scattered from the direct sunshine.

At points in the sky near the sun or antisolar point the polarization of the light scattered from the direct sunshine is very weak, according to Lord Rayleigh's laws, explained above, and here we find that in the vertical plane through the sun it is overpowered by a residual horizontal polarization in the subsidiary light just mentioned. Thus are formed the neutral (*i. e.* unpolarized) points discovered by Arago, Babinet, and Brewster. Bosanquet (Phil. Mag. July 1876) has investigated the direction of the polarization in the neighbourhood of the neutral point, and his results point to this conclusion. He found for instance that a little to the right or left of a neutral point the polarization was inclined at forty-five degrees to the vertical. Let us see if our explanation leads to this result. Neglecting the unpolarized part of the subsidiary light, we have at the neutral point two equally bright beams polarized at right angles to each other. Just to the right or left the beams are still equal, but the planes of polarization are not quite at right angles, and it is obvious from symmetry that the resultant polarization must bisect the angle between them.

An explanation of the residual polarization of the subsidiary light is not far to seek. Take the light scattered by a particle A in the horizontal direction AB. Light reaches the particle from the sky and the earth. The ground below may be roughly regarded as appearing equally bright in all directions, and therefore, as I proceed to show, it will produce no polarization. If A were in the centre of a uniformly bright spherical envelope, the light scattered along AB would be obviously unpolarized. The light from the upper and lower hemispheres must from symmetry be polarized in the same way, and, since the sum is unpolarized, each must be unpolarized. The lower hemisphere is equivalent to the uniformly bright surface of the ground; so my point is established. There remains therefore the light from the sky to A to be considered. For obvious reasons the sky is much brighter near the horizon than in the zenith, and it is clear that the light scattered along AB from any point in the

horizon (except exactly in the line AB) is more or less polarized in a horizontal plane. Hence the resultant of all this subsidiary light is slightly polarized in a horizontal plane. The case is not very different when AB is inclined somewhat downwards. As to the strength of this residual polarization I shall have something to say later.

Since the imperfection of the polarization at the maximum point is due to light reaching the particles from the earth and sky, we ought to find a great effect produced, as Lord Rayleigh has suggested, by the presence of snow on the ground. Again, at high altitudes above sea-level, owing to the darkness of the sky, the polarization should be more perfect. During the past year, having been resident for the most part at tolerably high altitudes, I have taken a good many measures of the maximum polarization with the object of examining these two points. Though the measures are rough, they are, I feel sure, free from large error. This is more than I can say for any other measures of the kind that I have come across, as I shall now explain.

Criticism of Brewster's and Rubenson's Measures.

The most extensive series of measurements on record are those of Brewster and Rubenson. Both used a pile of thin glass plates, tilted at such an angle as to depolarize the sky light, and tested the depolarization with a Savart polariscope. Brewster* seems to have relied on the formula :—

Ratio of principal intensities after passing through plates

$$= \cos^{4n}(\iota - \iota') \times \text{ratio before entering, where } n \text{ is the}$$
 number of plates, and $\iota \iota'$ are the angles of incidence and refraction.

This can be readily derived from Fresnel's expressions for the refraction of polarized light, but it only applies to the portion of light that is refracted directly through the plates. Besides this there are other portions, twice reflected, four times reflected, &c. which ultimately get through the plates.

* Phil. Mag. Aug. & Sept. 1865. Brewster does not fully describe in these two papers how he calculated the polarization from the readings of his polarimeter. He refers to other articles which I have not had the opportunity of looking up. But a little further on he quotes a formula equivalent to the above (with a misprint of n for $2n$) without any reservation. Besides he obtained values of the polarization which are quite extravagant. He uses the measure R where $\tan^2(45^\circ - R) = \text{ratio of principal intensities, which I call } r$. At mid-day on June 10th, 1841, at St. Andrews, he found $R = 30\frac{1}{4}^\circ$; whence $r = .07$. While to judge from Rubenson's observations, which I have analysed below, it seems in the highest degree unlikely that r was really less than .20.

With ten or twenty plates the number of these portions is very great and, though they are individually faint, the aggregate is considerable. Thus, of light polarized in the plane of incidence about seven times as much actually traverses twenty plates as is directly refracted through, when the angle of incidence is forty degrees. But of light polarized in the rectangular plane, that traverses the same plates, the reflexions only furnish about one seventh part. In the case of ten plates with an angle of incidence of fifty degrees, the corresponding proportions are three and a third times as much and an infinitesimal fraction. Brewster used from six to twelve plates and Rubenson twenty.

Rubenson* avoided Brewster's error, but fell into another connected with the reflexions. He determined not to rely on theoretical calculations of the polarizing power of the plates, especially as it would have been difficult to measure the index of refraction with sufficient accuracy, and he employed an excellent method, due to Arago, of standardizing his instrument, *i. e.* measuring its depolarizing power for various positions of the pile. He took a fairly thick plate of quartz, cut parallel to the axis, and placed it with its principal plane coincident with the plane of incidence on the pile. Behind this he put a Nicol prism attached to the vernier of a graduated circle. Let the plane of polarization of the light incident on the quartz make an angle ϕ with the principal plane; then the light emergent from the quartz is partially polarized, and the ratio of the principal intensities is $\tan^2\phi$. To secure complete depolarization by the pile of plates, it is essential that there should be no definite phase relationship between the two components of the partially polarized light. If the quartz plate had been thin this difficulty might have occurred; but in a fairly thick plate the phase relationship is so completely changed by a slight alteration of wave-length that, except with homogeneous light, the two components are practically independent.

So far, then, his method was admirable, but he never seems to have remembered that the reflected light which comes through the plates is more or less thrown to one side and that, therefore, the aperture which admits light must be large. If it be small only a proportion, and that a very uncertain proportion, of the reflected light reaches the eye. Rubenson had a large aperture in his observations on the sky and a small one when he was standardizing. The Nicol

* *Mémoire sur la Polarisation de la Lumière Atmosphérique*, par Dr. R. Rubenson. Upsal. 1864. To be had at Klemming's Antiquariat, Stockholm.

itself limited the aperture considerably, and, to stop light reflected from the sides of the Nicol, he put a diaphragm between it and the quartz. The diameter of the aperture, according to the engraving which he gives, was less than half the thickness of the pile of plates. I have tried to approximate to the amount of error involved, and have convinced myself that it must have been considerable. He himself was led to suspect some fault in his standardizer for the following reason:—Let us call the reading of the plates when they neutralized the polarization of the sky on a certain occasion G_1 . Then, when the plates were tilted in the opposite direction, they again neutralized the polarization at a reading G_2 . The absolute values of the polarization corresponding to the readings G_1 and G_2 should of course have been identical, but according to the standardizer they were always markedly different (*e.g.* such as to give $r = \cdot 250$ and $r = \cdot 225$)*. He was unable to find any explanation of these discrepancies, but they led him to regard his observations as only comparable with each other when the pile had not been dismounted in the interval. Further he specially says (p. 56) that his measures of the polarization are not to be compared with measures obtained with other instruments, or, in other words, that they are not absolute values at all. We can see now that he was right, and that these discrepancies were only a sign of a much larger error. Owing to some slight want of symmetry, more of the reflected light must have been thrown out of the field of view on one side than on the other.

When I first appreciated the meaning of this oversight in Rubenson's work, I thought it would render his elaborate series of measures quite useless except for intercomparison. But I have since discovered that the polarizing power of a pile of glass plates varies very slowly with the index of refraction. I do not know how this result will strike others, but to me it was most unexpected. However, it enables us to obtain a fair idea of the real values of the polarization that Rubenson observed. There is a very simple formula, due to Prof. Stokes†, for the polarizing power of a pile, taking into account all the reflexions. Once found it may be easily verified. Let a be the proportion of light, polarized in the plane of incidence, reflected from a single surface; then the

* He aimed at a probable error less than $\cdot 002$, and would no doubt have succeeded if the error had not been systematic.

† Proc. Roy. Soc. 1862. It was rediscovered by Prof. W. G. Adams, Phil. Mag. March 1871.

proportion of light, polarized in the plane of incidence, which traverses a pile of n plates is

$$\frac{1-a}{1+(2n-1)a}.$$

A similar formula holds good for the light polarized at right angles to the plane of incidence. In this case let us write b for a . Thus if a broad beam of ordinary light enter a pile of plates, the ratio of the principal intensities in the emergent beam is

$$\frac{1-a}{1+(2n-1)a} \quad \frac{1+(2n-1)b}{1-b}.$$

So far the formulæ are perfectly general and independent of the truth of any particular laws of the reflexion of polarized light. To proceed further we must assume certain values of a and b . Now Fresnel's laws are known to be at least approximately true, and these give

$$a = \frac{\sin^2(\iota - \iota')}{\sin^2(\iota + \iota')}, \quad b = \frac{\tan^2(\iota - \iota')}{\tan^2(\iota + \iota')}.$$

Using these values I have calculated the following figures for the ratio of the intensities in the beam emergent from twenty plates when the incident light is unpolarized :—

Angle of Incidence ..	30°.	40°.	50°.
$\mu = 1.52$580	.360	.182
$\mu = 1.56$562	.355	.174
By Rubenson's Standardizer	.532	.293	.120

I presume that we may safely assume that Rubenson's plates (of which he gives no special description) had an index of refraction nearer 1.52 than 1.56. The most uncertain point seems to be the truth of Fresnel's laws. In working out the following results from Rubenson's figures I have assumed their truth as well as the index 1.52. It must be distinctly understood that these results are only rough approximations.

There are several different methods of expressing the strength of partial polarization, and it is not easy to choose between them. If A, B be the intensities of light polarized in the two principal planes, of which B is the lesser, the most natural expression seems to be the simple ratio B/A . I shall call this r . In completely polarized light $r=0$, in unpolarized light $r=1$. But in the special subject of this inquiry it will be often convenient, though not very accurate, to regard sky light at the maximum point as composed of some completely polarized light, $A-B$, consisting of scattered sunlight, and of some unpolarized light $2B$, consisting of

scattered earth and sky light. I shall therefore denote the ratio $2B/(A-B)$ by the symbol s . In completely polarized light $s=0$, and in unpolarized light $s=\infty$. In the time of Rubenson it seems to have been the established custom to call the ratio $(A-B)/(A+B)$ of the polarized light to the whole light "the polarization." This expression has the advantage that it increases with the strength of the polarization and vanishes when the light is unpolarized. But it seems to be for most purposes inconvenient, and I shall not use it at all.

Results deduced from Rubenson's Work.

Rubenson's observations were made at Rome, and may therefore be taken as a fair sample of the polarization at the sea-level. Rome is sufficiently far (some thirteen miles) from the sea to prevent serious disturbance from the reflexion on the water. He found the polarization strongest in the morning and evening, attaining a maximum about mid-day, sometimes before and sometimes afterwards. About mid-day the rate of alteration is of course slow, and he noticed that slight perturbations, *i.e.* sudden irregular variations, were most frequent about that time. The increase of polarization became more and more rapid towards sunrise or sunset. He has collected into a table his readings on many different days at noon and at sunrise or sunset, in summer and winter. Between May 22nd and July 27th, 1862, the strongest mid-day polarization was (July 5th) $r=.276$ ($s=.76$), while there were ten days with r less than $.34$ ($s<1.03$). With the sun on the horizon the strongest polarization was $r=.218$ ($s=.56$), while there were seven days on which r fell as low as $.25$ ($s=.67$). In winter the polarization was much stronger. Between Oct. 21st, 1861, and Jan. 8th, 1862, there was one noon (Nov. 5th) at which r was $.20$ ($s=.50$), while on ten occasions r at noon was less than $.25$ ($s<.67$). With a horizontal sun the strongest polarization was given by $r=.156$ ($s=.37$), and there were eleven days on which r was less than $.19$ ($s<.47$). Not much difference is noticeable between sunrise and sunset.

I have only cited the days on which the polarization was strongest, for these always have the most perfect skies, as any observer must very soon convince himself. Any modifying circumstances, such as haziness of the sky, smoke, the faintest cloud in the field of view, the presence of large clouds scattered about the sky, serve invariably to diminish the polarization. The increase of s , as the sun mounts from the horizon to its highest altitude, in summer from $.56$ to $.76$,

and in winter from $\cdot 37$ to $\cdot 50$ is clearly due to the increase in the earth light. But it is not so easy to explain the difference between winter and summer with a horizontal sun. It must depend on some material difference in the atmosphere. It is possible that the brightness of the sky is augmented in the ratio 56 to 37, owing to an increase either in the number of the fine particles or in their average size. The size is particularly important, as the light scattered by a fine particle is proportional to the square of its volume; so a little moisture deposited on dust particles would increase their light-giving power enormously. On the other hand, the difference might be due to the sky never being as pure in the summer as in the winter. The presence of dust particles larger than a wave-length would of course depolarize the scattered light.

Rubenson gives a few observations at Rome of the polarization in the vertical plane at distances from the sun other than 90° . They were taken for the sake of fixing the maximum point, but they are interesting to us for another reason.

Rome, June 21st, 1861.

Hour.		Solar Distance.	r .	Zenith Distance.	
h	m			$^\circ$	'
9	15 A.M.	90°	$\cdot 288$	51	9
9	26 "	120	$\cdot 490$	83	35
9	43 "	60	$\cdot 500$	26	0
9	57 "	90	$\cdot 297$	58	27
3	26 P.M.	90	$\cdot 277$	43	59
3	43 "	110	$\cdot 382$	60	50
3	58 "	70	$\cdot 380$	17	34
4	19 "	90	$\cdot 279$	34	24

These are all in the vertical plane through the sun. Let us compare these values at 60° and 120° with those deduced from Rayleigh's laws on the assumption that the particles are all small. We must presuppose something about the polarization of the subsidiary light. Now, from Brewster's observations at St. Andrews, the neutral point below the sun (called after his name) is at an angular distance of about 10° , when the sun is at 54° above the horizon. This supplies what we want, at least approximately. Let u, v be the components of the light which reached the particles directly from the sun, and x, y the components of the subsidiary light, polarized in the vertical plane and in a plane at right angles thereto respectively. At the neutral point we have

$$y - x = u - v = u - u \cos^2 10^\circ = \cdot 03u.$$

This gives the polarization of the subsidiary light below the sun at a zenith distance of 46° . It is rather loose reasoning to apply this result to points opposite to the sun at zenith distances of 26° , 55° , and $83\frac{1}{2}^\circ$; but the consequent errors will probably be small and should have opposite signs in the first and third cases. Taking the mean of the readings at 90° , we have

$$y = .293(u + x).$$

From these two equations we have $x = .373u$, $y = .403u$ at 90° from the sun. At 60° and 120° we have

$$r = \frac{v + y}{u + x} \quad \text{and} \quad v = u \cos^2 60^\circ.$$

Inserting the values of x and y we find $r = .477$, which agrees with the observed values of r quite as well as could be expected, considering the uncertainty of my interpretation of his readings. A similar process applied to the afternoon observations gives at 70° and 110° $r = .366$.

Description of Apparatus.

The arrangement of my polarimeter will be understood from Pl. VII. fig. 1. The light entered through the aperture A, and traversed the movable pile B of five glass plates, the fixed pile C of three glass plates, and the thick plate D of Iceland spar, cut at right angles to the axis. It was then reflected by the mirror E, and reached the eye through the Nicol F. Thus B and C depolarized the light, while the depolarization was tested with the polariscope DEF. On turning the five plates the black cross became gradually fainter and finally disappeared, being soon replaced by four black spots. I took the reading halfway between the disappearance of the cross and the appearance of the spots. These spots were of course the spaces between the white cross and the first white ring. The optic axis of D gave a fixed direction for the observed light, so the angle of incidence on the plates C remained constant so long as C and D were undisturbed. It was about 50° . The plates B were fastened to two uprights *a b*, projecting from a round disk of tin plate, dotted in the figure, resting on the side of the box. From the centre of the disk a spindle passed through the side of the box carrying a pointer *c*, shown in fig. 2, by whose means the plates could be rotated about the spindle and their angular position read off. The instrument was roughly made and had various defects. The Nicol and Iceland spar I got from England, but the rest was made either by myself or by such workmen

as I could find in these Alpine villages. The sensitiveness of the polariscope was no doubt seriously diminished by the insertion of the mirror E, which was a piece of ordinary looking-glass. Of course it ought to have come between the Nicol F and the eye. To add to the difficulties of reading, there were some large dents on the faces of the Iceland spar.

These things must be my excuse for not reaching the same degree of accuracy in reading as others have. Still I seldom found two readings of the same thing differ by more than 2° , which corresponds roughly to a change of 0.02 in the ratio r . For each observation I took four readings of the angle of incidence, two on one side of the zero and two on the other. So the probable error of the mean is certainly not greater than 1° . The object of separating the glass plates into two piles was to increase the power of the instrument by diminishing the amount of transmitted reflected light. Thus, for an angle of incidence of 50° on the five plates, my arrangement gave $r=0.25$, while eight plates in one pile would for the same angle of incidence only have given $r=0.34$. These values are calculated, not observed. The plate of Iceland spar was about 23 millim. thick. This great thickness was of importance, bringing the four black spots near together.

It was necessary, not merely to be able to point the polarimeter to the required part of the sky, but also to make the plane of rotation of the glass plates pass through the sun. This was secured by the mounting shown in fig. 2. The polarimeter was fastened with a screw-bolt G to a board, hinged at HH to another board, itself fastened with the screw-bolt K to the head of a tripod belonging to a photographic camera. By rotations about K and H it was easy to point the edge *ed* to the sun. Then, by turning the polarimeter about the bolt G, observations could be taken of any point of the sky distant 90° from the sun. With the same mounting I could have observed the polarization at any point of the vertical plane through the sun. But I have not done so hitherto, chiefly because it would have required accurate pointing of the instrument and another graduated circle. Since the polarization reaches a maximum about 90 degrees from the sun, an error of two or three degrees in the pointing is of no consequence there. Figure 2 shows how the position of the Nicol, and therefore of the mirror E, added much to the convenience of the observer as well as to the simplicity of construction, though it was prejudicial to the sensitiveness of observation.

Observations at St. Moritz.

Most of my observations were taken at St. Moritz during the autumn of 1887. For a proper appreciation of them some description of the country is necessary. The Engadine may be roughly represented by a straight nearly level trough, running from south-west to north-east, whose steep sides are about three thousand feet high. On either side of the trough, especially on the south, there are extensive mountain-ranges. The place of observation, viz. the Kulm Hotel, is situated on the northern slope, about 300 feet above the bed of the valley and 6000 feet above sea-level. Immediately below, the bed of the valley is occupied by a lake above half a square mile in area. On October 20th the snow covered the opposite slope, coming right down to the water's edge, though the uniform whiteness was a good deal broken by woods. The northern slope was nearly free from snow up to perhaps 8000 feet above sea-level.

All the observations refer to points ninety degrees from the sun, and, unless the contrary is stated, to the highest of such points in a cloudless sky. The chief results are numbered 1, 2, 3, 4. G denotes the angle of incidence on the five plates when neutralization takes place; r and s are different measures of the polarization defined above. The manner in which they are derived from G is explained below under the heading Standardization. The error in r , due to bad setting of the pile, is not likely to exceed $\cdot 01$ except when G is greater than 55° , and the probable error in standardization is of about the same magnitude.

1. The polarization is weakest towards noon.

October 21.

		G .	r .	s .
9.0	A.M.	53°	$\cdot 209$	$\cdot 53$
9.20		$51\frac{1}{2}$	$\cdot 221$	$\cdot 57$
12.55	P.M.	$50\frac{1}{2}$	$\cdot 228$	$\cdot 59$
3.50	Too great to be measured. Sun just setting behind hill.			
3.55		$61\frac{1}{2}$	$\cdot 156$	$\cdot 37$ Inclined to right.

The last measure was only a rough estimate, as the polarization was so strong. The instrument was pointed over the mountains to the right, which were in full sunshine. The polarization then was not so strong as at the highest point (see 2).

October 22.

	G.	r.	s.	
3.36 P.M.	$60\frac{1}{2}^{\circ}$	·168	·38	Near the hill to the left.
3.40	$59\frac{3}{4}$	·165	·40	„ „ „

At this time the polarization at the highest point was too strong to be measured.

After this I seldom took readings except near the middle of the day.

2. The effect of the ground being covered with snow is to materially diminish the polarization. Thus add to the readings just quoted the following :—

		G.	r.	s.
October 22.	9.0 A.M.	$52\frac{1}{4}^{\circ}$	·215	·55
	9.15	$51\frac{1}{2}$	·221	·57

and then compare them with the readings a few days later after five inches of snow had fallen :—

		G.	r.	s.
October 26.	10.15 A.M.	41°	·318	·93

On this day there were a few small clouds near the horizon, not enough to produce any appreciable effect.

The hot sun swept away the fresh snow very quickly, and by the next morning the greater part of it had gone.

		G.	r.	s.
October 27.	11.40 A.M.	$47\frac{3}{4}^{\circ}$	·251	·67

Again, after some bad weather, during which three feet of snow fell, came a fairly good day.

		G.	r.	s.
November 6.	10.0 A.M.	37°	·366	1·16
	10.6	$36\frac{1}{4}$	·377	1·21

The point of observation was perfectly clear, though there were a few scattered clouds about. An hour later the sky was nearly covered with clouds, so the weak polarization on this day may be partly attributed to that circumstance (see 4). I will therefore cite also :—

		G.	r.	s.
November 13.	12.50 A.M.	41°	·318	·93
	12.57	$42\frac{1}{2}$	·301	·86

The sky was cloudless at the time of observation and nearly so all day. The white covering on the ground was more broken than on the 6th, and the snow had gone from the trees.

After this date the snow covering was practically perfect except for the dark trees. The previously dark lake was

hidden on November 27 by a white mist, and in January by both ice and snow. I quote the following readings:—

		G.	r.	s.
Nov. 27.	11.6 A.M.	$38\frac{1}{2}^{\circ}$	·345	1·06
	11.30	40	·326	·97
Jan. 4.	12.37	$42\frac{1}{2}$	·301	·86
	17. 1.0 P.M.	41	·318	·93

3. The polarization at different points, distant 90° from the sun, is affected by the brightness of the ground below. The following successive observations were taken on a cloudless day:—

October 21.

	G.	r.	s.	
9.0 A.M.	53°	·209	·53	At the highest point.
	$55\frac{1}{4}$	·191	·47	Near hills to the right.
	$51\frac{1}{2}$	·221	·57	At the highest point.
9.30	$53\frac{1}{2}$	·205	·51	Near hills to the right.

The altitude of the points observed in the second and fourth of these readings may have been about 5° . In this instance the polarization was materially stronger near the horizon. This was due to the instrument being pointed down the long straight valley, which was nearly clear of snow and was bordered by dark woods. Later on in the same day this general principle led to an opposite result.

October 21.

	G.	r.	s.	
12.55 P.M.	$50\frac{1}{2}^{\circ}$	·228	·59	At the highest point.
	$47\frac{1}{4}$	·257	·69	Near hills to the left.
1.10	48	·249	·67	Near hills to the right.

The weaker polarization in the two last observations was due to the line of sight passing over neighbouring and far-reaching tracts of snow. On the following day similar results were obtained.

October 22.

	G.	r.	s.	
8.55 A.M.	$54\frac{1}{4}^{\circ}$	·199	·50	Near hills to the right.
9.0	$52\frac{1}{4}$	·215	·55	At the highest point.
9.7	$54\frac{1}{4}$	·199	·50	Near hills to the right.
9.15	$51\frac{1}{2}$	·221	·57	At the highest point.

4. The formation of clouds seemed to be heralded by a

weakening of the polarization. I found later that the same thing had been noticed by Rubenson. On October 20, at 10 A.M., at the highest point in a cloudless sky, the reading was 49° , while, judging from the two succeeding days, it should have been $51\frac{1}{2}^\circ$. By 11.45 A.M. the sky was nearly covered with cirrus, but through a clear rift near the highest point I obtained the reading $45\frac{1}{4}^\circ$. It seems probable that the ice particles of the cirrus were present in considerable numbers at 10 A.M., but, though as thick or thicker than a wave-length, they were too small and too few to be visible as cloud.

Generally speaking the polarization was much the same on two successive cloudless days, unless there had been a manifest change in the whiteness of the ground. This is very different from Rubenson's experience, but is only what might be looked for in the clear atmosphere of the high Alps.

Observations at Thusis and Davos.

During the spring other work intervened, and, indeed, owing to the bad weather, there were few opportunities, so that I did not take any more readings till after leaving St. Moritz early in April for Thusis. During the journey the Iceland spar unfortunately got shifted, but I was able to measure the consequent alteration in the angle of incidence on the three fixed plates in the following manner:—At St. Moritz the average difference of the readings on the right and left sides of the zero was about 2° . At Thusis it was about 6° . To produce this change the axis of the crystal must have been shifted through 2° , and the angle of incidence on the fixed plates must have been diminished by 2° . It is true that for the journey I dismounted the movable pile; but, owing to the method of attachment, it must have been replaced in practically the same position with respect to the pointer of the graduated circle. Further on I shall explain how I made allowance for this change of 2° . Since the observations at Thusis nothing in the polarimeter has been disturbed.

The village of Thusis (2450 feet above sea-level) lies at the southern end of a broad open valley running due north. To the south there are high wooded cliffs separated by the narrow cleft of the Via Mala. Here I had another opportunity of testing the effect of snow on the ground. Two or three days of heavy snow were succeeded by a glorious day. At first it was cloudless, but by 11.30 A.M. a few small clouds appeared to the west.

April 14, 1888.

	G.	r.	s.	
10.10 A.M.	36°	·399	1·33	At highest point.
10.20	37	·385	1·25	" "
10.40	36	·399	1·33	" "
10.50	34	·427	1·49	Towards east, altitude 25°.
11.0	30	·485	1·89	Towards west, altitude 35°.
11.30	35½	·406	1·37	At highest point.

At the time of these readings the snow, though rapidly yielding to the sun, still covered the valley for a mile from Thusis ; further north it had gone from the lower slopes as well as the valley-floor, so that to east and west the country was very white, while to the north there was a long stretch of brown and to the south there lay the black woods. Thus the snow was placed advantageously for diminishing the polarization about noon. It is interesting to note the diminution of the polarization looking over the snow to east and west. The altitudes given were estimated by eye.

Two days afterwards the snow had gone for some distance up the slopes, especially to the east, but the sky was not so perfect ; there were a good many small cumuli near the horizon.

April 16.

	G.	r.	s.	
11.0 A.M.	38½°	·365	1·15	At highest point.
11.30	38¾	·361	1·14	" "

Later on the clouds spread over the sky.

The next day the sky was practically cloudless and the snow had gone rather more.

April 17.

	G.	r.	s.	
10.7 A.M.	40°	·345	1·06	At highest point.
10.15	34¾	·416	1·43	To west, altitude 30°.
10.23	40	·345	1·06	At highest point.
10.30	38½	·365	1·15	" "
10.35	39	·358	1·12	" "
11.0	35	·413	1·41	To east, altitude 25°.
12.15	38¼	·369	1·17	At highest point.

One may say, therefore, that the extra snow on the ground on the 14th as compared with the 17th reduced the reading at 10.10 from 40° to 36° and at 11.30 from 38½ to 35½°. Of course on the former day a good deal of snow disappeared between 10 A.M. and 11.30 A.M.

On the evening of the 17th, finding the polarization more within the power of the instrument than it had been at St. Moritz, I followed it up to sunset.

April 17.

	G.	<i>r.</i>	<i>s.</i>	
4.25 P.M.	$52\frac{1}{2}^{\circ}$	·224	·58	At highest point.
5.20	54	·212	·54	A few clouds near horizon.
About 5.30 the sun disappeared behind the hill at an altitude [of, perhaps, 10° .				
5.33	$56\frac{1}{2}$	·195	·48	

Soon after this a bank of clouds came up over the place when the sun had disappeared, and by 5.45 the shadow had crept up to a height of 1200 feet on the hills close by.

	G.	<i>r.</i>	<i>s.</i>
5.45 P.M.	$56\frac{3}{4}^{\circ}$	·193	·48
6.5	57	·192	·47
6.20	$59\frac{1}{4}$	·178	·44

By 6.20 a good many small clouds had come over the sky. At 6.25 both the clouds and all the mountains in sight were in shade, and the polarization was too strong to be read. I estimated it at $G=64^{\circ}$, $r=.15$, $s=.35$.

I have taken a few observations at Davos during the summer, altitude 5100 feet above sea-level. The situation is not unlike that of St. Moritz.

	G.	<i>r.</i>	<i>s.</i>	
May 12, 12.30 P.M.	38°	·372	1·18	At highest point.

Sky practically cloudless. At this date the snow was still lying on the south side of the valley down to the level ground, but the north side was tolerably clear.

	G.	<i>r.</i>	<i>s.</i>	
June 2, 12.30 P.M.	47°	·334	1·00	At highest point.

Sky practically cloudless. The snow came down to within 2000 feet of the valley-floor on the south side.

	G.	<i>r.</i>	<i>s.</i>	
Aug. 9, 1.0 P.M.	58°	·185	·45	Sky cloudless.

Scarcely any snow to be seen on the mountains.

	G.	<i>r.</i>	<i>s.</i>	
Sept. 23, 10.0 A.M.	61°	·167	·40	Sky almost cloudless.

Probably even less snow on the mountains.

G. *r.* *s.*
 Nov. 27, 11.47 A.M. $45\frac{1}{2}^{\circ}$.285 .80

This was a few days after twenty inches of snow had fallen, and the covering was fairly complete except for the bare trees. There were a few clouds about, but none near the point of observation.

G. *r.* *s.*
 Dec. 2, 12.5 A.M. $44\frac{1}{2}^{\circ}$.295 .84 Sky almost cloudless.

This was after some fresh snow. The covering was rather more complete and the trees were slightly sprinkled with snow.

January 2, 1889.

G. *r.* *s.*
 12.42 P.M. 53° .220 .57. Sky cloudless.

At this time there was unusually little snow for the season of the year. The north side of the valley was in great part bare even up to the mountain-tops. Though the snow-covering was more complete than on May 12, the value of *s* was less than half that on the former date. This of course was due to the low altitude of the sun.

Comparing the values of *s* on Aug. 9 and Dec. 2, we see that the subsidiary light is nearly doubled by the replacement of the green grass by the white snow, notwithstanding the much brighter light on the former. It seems fair to conclude that the greater part, at least three quarters, of the subsidiary light in December comes from the snow. The ratio of the subsidiary light to the whole being 84 : 184, we deduce that, of the light from the sky near the point of maximum polarization, at least one third has been previously reflected from the snow on the ground.

It must be only too obvious to a reader of my observations that I was much hampered by the limited power of my instrument. It was barely able to deal with the polarization in the middle of the day at St. Moritz and Davos, so that we are still in ignorance of the strength of the polarization at sunrise or sunset. This is unfortunate, for the most striking difference between the polarization at high altitudes and at the sea-level would naturally be when the earth-light was weakest. I am taking steps, therefore, to get a more powerful instrument. Meanwhile we may notice that, even at noon, the difference is in favour of the high altitudes when the ground is nearly free from snow. The strongest mid-day polarization found by Rubenson at Rome was $r = .20$ on Nov. 5. Compare this with $r = .185$ at Davos on Aug. 9. I should not like to lay much stress on the slight difference between these numbers ; but, even if they were equal, it is to

be remembered that at Davos the sun was much higher (57° instead of 32°), and the ground consequently more than half as bright again.

Any marked deviation of the plane of polarization from the plane through the sun was shown in my instrument by the black cross gradually being transformed into the white one without even disappearing. While the point nearest neutralization was being passed, the black cross was rotating through forty-five degrees and changing its appearance, so that the four arms of the black cross became the dark spaces between the arms of the white cross. I believe that with proper precautions very accurate measurements of the position of the plane of polarization of partially polarized light might be made on this principle. I found that, with large clouds about, the deviation might reach, perhaps, three or four degrees; but in a clear sky, *e. g.* in all the measurements I have recorded, it was insensible. It was curious to find that it was easy to point out the position of the sun behind a mountain with considerable accuracy by applying the same principle. It was done thus. First putting the plane fde (fig. 2) vertical, I turned the instrument round the bolt K till there was no rotation of the black cross on neutralization. This insured the plane fde passing through the sun. Then, having turned the box about G till a point near the horizon was under observation, I opened or shut the hinge H, till fde again passed through the sun. Then the sun lay in the direction ed .

In the observations cited above I have measured the average polarization of all the different colours which go to make up the light of the sky. There remains the question how the polarization varies with the wave-length of the light. Since the imperfection of the polarization is due to light reaching the particles from the sky and earth as well as from the sun, we should expect the polarization to increase with the wave-length. For sky light is blue and earth light can hardly be redder than sunlight. I tried at Thusis the effect of interposing orange glass, which, according to my estimate, should have increased the reading G by several degrees, but did not find any decided change. I cannot account for this result. Though I hope to repeat the observations, I should like to call the attention of other workers to this point. The effect ought to be greater at sea-level on account of the increased brightness of the sky. Another theoretical result, which might be easily tested by any one with a suitable polariscope, is that the position of the neutral point should change considerably with the wave-length of the light under examination.

Standardization.

As has appeared frequently in what precedes, a great difficulty in these observations is to translate the readings of the pile into absolute measure. It is impossible at present to do this satisfactorily by calculation, for we do not know the laws of the reflexion of polarized light at the surface of glass with sufficient accuracy. Each instrument must therefore be standardized by direct experiment. Three methods have been proposed for doing this:—The first, by the aid of photography, was suggested to me by Capt. Abney, who has used platinotype paper for measuring the brightness of the sun and sky. It consists in exposing sensitive paper to the light from a small portion of the sky for measured intervals of time, interposing a Nicol prism in the path of the light. By placing the Nicol in the two rectangular positions, we could determine the ratio of the principal intensities, while simultaneous observations were being made with the optical polarimeter. Difficulties might occur in the light reflected from the sides of the Nicol and in the great length of exposure, and at any rate the method would require a good deal of preliminary testing. Bosanquet has proposed another method, viz. to mix common and completely polarized light in known proportions with the aid of a divided object-glass (*Phil. Mag.* Dec. 1875), and then take the reading with the polarimeter.

The simplest and most convenient method, and with proper precautions the most accurate, seems to be that invented by Arago and already described as being used by Rubenson, and it is the one I have adopted. Bosanquet throws some suspicion on the method as resting on an uncertain theoretical basis, but this suspicion is by no means deserved. The main assumption involved is that a beam of plane polarized light may be treated as composed of two beams, polarized in rectangular planes, of intensities proportional to $\sin^2 \phi$ and $\cos^2 \phi$, where ϕ is the angle between one of these planes and the original plane of polarization. This is a direct consequence of the principle of the superposition of small motions, which is universally admitted to be applicable to light. This assumption granted, we have only to add that in the thick plate of quartz one component is retarded with respect to the other by a large number of wave-lengths, and that, therefore, after leaving the quartz there is in homogeneous light no permanent phase-relationship between the components. The emergent light, then, is partially polarized in the principal plane of the quartz, and the ratio of the principal intensities is $\tan^2 \phi$. A certain amount of light is, of course, lost by reflexion at each

surface of the quartz, and a somewhat smaller proportion is lost of that polarized in the principal plane than of that polarized in the perpendicular plane; but the difference is very small. The former proportion is about 0.0460 and the latter 0.0472. These are for a single reflexion. Therefore, owing to the double reflexion, the $\tan^2 \phi$ should be multiplied or divided by the factor 1.0025. This correction is quite negligible in such work as the present.

My standardizer consisted of a wooden framework to hold the quartz plate, with a round hole over the quartz just large enough to let the brass tube of the Nicol turn freely. To the Nicol were attached two pointers for taking the readings on opposite sides of a carefully graduated circle. The framework was screwed onto the polarimeter, care being taken to set the principal plane of the quartz as nearly as possible parallel to the plane of rotation of the glass plates. The outer end of the Nicol was closed with a glass cap to keep out dust; but between the Nicol and the quartz or the quartz and the pile of plates no glass intervened. The quartz plate was nearly an inch thick. It was one of the pieces (plate iii.) I used in a research on the wave-surface of quartz (Phil. Trans. part i., 1886).

The readings were taken by setting the pile at a known angle of incidence and then adjusting the standardizer till the polarization was neutralized. This could be done in four different positions of the Nicol, corresponding to the four quadrants, since $\tan^2 \phi$ attains any given positive value four times while ϕ is changing from 0° to 360° . I graduated my circle suitably, two opposite points being called 0° , the two rectangular points 90° , and four intermediate points 45° . Averaging the readings in adjacent quadrants eliminated any error in the position of the zero-point, while averaging the readings in opposite quadrants eliminated, at least approximately, the error arising from the axis of rotation of the Nicol not being truly parallel to the incident light*. I did not take any large number of readings, as it seemed not worth while to aim at any high degree of accuracy in standardizing observations of such a variable character as those I have given. Moreover, in optical observations there is always some risk of the errors being systematic. In the following Tables are given the means of the readings in the four quadrants for different values of the angle of incidence G on the five plates. The upper line gives the readings when the plates were tilted to the right, *i.e.* the same way as the fixed plates, the lower

* See a paper on the use of Nicol's prism, Phil. Mag. May 1885.

when tilted to the left. In the first determination I used sky light, but, finding that on Fresnel's theory the depolarizing power of the pile of plates was almost independent of the colour of the light, I decided in the second determination to avail myself of the much greater brightness of sunlight reflected from a sheet of white paper.

Aug. 13, 1888.—Sky light, Davos.

G.....	0°.	30°.	40°.	50°.
ϕ	37°·7	34°·7	30°·3	27°
ϕ	39°·3	35°	31°·2	25°·5

Sept. 14 and 15, 1888.—White light.

G	40°.	50°.	60°.
ϕ	30°·1	26°·8	22°·7
ϕ	29°·9	26°·1	21°·0

These various readings are not as accordant as I should have wished. The probable error of the figures given is nearly twice as great as that of single observations of the sky. This increased uncertainty arose from the smallness of the field of view in the standardizer. The aperture of the polarizing Nicol was about 15 millim. by 10 millim., and its distance from the eye about 33 centim. On the other hand, in taking a reading at a high angle of incidence, such as $G=60^\circ$, the standardizer had an advantage in that there was no difficulty in getting a complete reversal, changing the black cross into white, or *vice versâ*. Whereas in taking an observation of a strongly polarized sky, the plates had to be moved, and this difficulty always occurred. For this reason observations of the sky for which G approaches or exceeds 60° are somewhat untrustworthy.

It will be noticed that I had to follow Rubenson's example in using a much smaller aperture for standardizing than for observing. I used a pile of five plates only * while Rubenson

* My pile of five plates was about 10 millim. thick, which is less than the aperture of the Nicol in the direction which was of importance; Rubenson's pile was apparently more than twice as thick as his aperture was wide.

used twenty, so that, *à priori*, the resulting error might be expected to be much smaller in my case. Still I thought it desirable to apply some test to see how great the error was. So I prepared a diaphragm with an aperture about half the apparent breadth of the Nicol, and took observations of the sky with and without the diaphragm, with the five plates inclined the same way as the three. The observations with such a small aperture were very difficult; but I satisfied myself that the change produced by the diaphragm, at $G=50^\circ$ did not exceed $2\frac{1}{2}^\circ$, and was probably less. With the actual aperture of the Nicol the error is certainly less than 1° , and when the two plates are inclined the other way the error should be negligible. Thus the mean error should be less than $\frac{1}{2}^\circ$, and for $G=50^\circ$ I have made no correction. For $G=60^\circ$ it was impossible to determine the error in this way. But some correction is probably necessary. So I have added $\cdot 01$ to the value of r , being partly guided by the value of r deduced from Fresnel's laws.

In the following Table the first line gives the mean observed value of ϕ , the second the corresponding value of $\tan^2 \phi$, i. e. r , the third and fourth the greatest and least observed values of $\tan^2 \phi$, the fifth the value of r calculated from Fresnel's laws on the assumption that $\mu=1\cdot52$, and the sixth the value of r finally adopted. The angle of incidence on the three plates was found by a careful measure to be $47\frac{1}{2}^\circ$, and this angle was used in the calculation.

G	0°	20°	30°	40°	50°	60°
ϕ	$38^\circ 5$...	$34^\circ 9$	$30^\circ 4$	$26^\circ 3$	22°
Mean observed r ...	$\cdot 630$...	$\cdot 485$	$\cdot 345$	$\cdot 244$	$\cdot 163$
Greatest observed r .	$\cdot 669$...	$\cdot 489$	$\cdot 368$	$\cdot 260$	$\cdot 175$
Least observed r ...	$\cdot 598$...	$\cdot 481$	$\cdot 330$	$\cdot 228$	$\cdot 147$
Calculated r	$\cdot 603$	$\cdot 541$	$\cdot 457$	$\cdot 370$	$\cdot 267$	$\cdot 188$
Adopted r	$\cdot 630$	$\cdot 571$	$\cdot 485$	$\cdot 345$	$\cdot 244$	$\cdot 173$

The intermediate values were obtained by interpolation, using second differences where necessary. By calculating on Fresnel's theory the values of the ratios for three plates under the incidences $47\frac{1}{2}^\circ$ and $49\frac{1}{2}^\circ$ respectively, I found that to make this Table apply to the readings at St. Moritz five per cent. had to be subtracted from the values of r .

The discrepancy between the observed values and those calculated from Fresnel's laws seems to exceed probable errors

of experiment. I should not like to press this point, but the method, if carried out with suitable care and precautions, might be usefully applied to test Fresnel's values of a and b , especially the former. For instance, by using first a pile of five plates and then a pile of eight, two independent equations in a, b would be obtained for each angle of incidence. The plates should be thin and the Nicol large.

Davos Platz, Jan. 4th.

XIII. *On the Suppressed Dimensions of Physical Quantities.*

By A. W. RÜCKER, M.A., F.R.S.*

IN the calculation of the dimensions of Physical quantities we not unfrequently arrive at indeterminate equations in which two or more unknowns are involved. In such cases an assumption has to be made, and in general that selected is that one of the quantities is an abstract number. In other words the dimensions of that quantity are *suppressed*.

The dimensions of dependent units which are afterwards deduced from this assumption are evidently artificial, in the sense that they do not necessarily indicate their true relations to length, mass, and time. They may serve to test whether the two sides of an equation are correct, but they do not indicate the mechanical nature of the derived units to which they are assigned. On this account they are often unintelligible.

Another difficulty is caused by the fact that the units which we find it convenient to use are sometimes themselves of an artificial character, and not such as would have been chosen had those who originally defined them possessed a clear conception of the nature of the phenomena with which they were dealing.

The obstacles which the study of the subject presents to students would, I think, be greatly reduced if in developing it symbols were retained in the formulæ to represent the quantities the dimensions of which are suppressed, and if the artificial appearance of the dimensions of many quantities were more clearly traced to the artificial character either of the quantities themselves or of the assumptions on which these dimensions are calculated.

It is probable that, as the mechanical explanation of physical phenomena proceeds, the use of units based on simple mechanical conceptions will be extended, that some quantities now employed will be dispensed with or regarded from a different point of view.

* Communicated by the Physical Society : read November 24, 1888.

Thus the satisfactory expressions of the dimensions of the various thermal units is not possible so long as there is any doubt as to the mechanical definition of temperature.

The equation

$$[M L^2 T^{-2}] = [J M c \theta]$$

leads, if we regard c as a number, to the result

$$[J] = [L^2 T^{-2} \theta^{-1}].$$

The quantity θ can hardly be taken as an abstract number, because the scale on which it is measured affects the numerical value of J , but, on the other hand, temperature as measured on the ordinary thermometer-scales has no relation to the units of length, mass, or time. No change in these units would affect the magnitude of the degree Centigrade or Fahrenheit. So long, therefore, as we adhere to the system of measuring heat in terms of calories instead of in ergs, temperature must be regarded as a fundamental quantity in the sense that the unit of temperature, though necessary for the expression of other thermal units, is itself independent of the units of length, mass, and time.

If it were sufficiently certain that the analogy offered by the theory of gases might be extended to all cases, it would follow that temperature depended only on mean energy of a particular kind (in gases the mean energy of translation) possessed by the molecules. The mean value of a quantity is of the same dimensions as the quantity itself, and thus temperature might be regarded not only as being measured by, but as being the mean energy of translation of a molecule, or of a standard number of molecules. The dimensions of temperature would thus be $[M L^2 T^{-2}]$. The number which expressed a given temperature would depend on the fundamental units, because they determine the magnitude of the unit of energy. It would be independent of the concrete mass or velocity of the molecules of any particular gas.

In the case of air at 0°C. and at atmospheric pressure $V=48,500$ centim. per second. If then, *exempli gratiâ*, the unit of temperature were that necessary to produce an increase of 1 erg in the mean total energy of translation of the number of molecules contained in 1 cub. centimetre of gas at 0°C. and at atmospheric pressure, the absolute temperature of melting ice on this scale would be

$$\frac{1}{2} \times .001293 \times 48500^2 = 1.5207 \times 10^6.$$

Absolute temperatures Centigrade could be converted into this scale with considerable accuracy by the multiplier $10^5/18$; for, dividing 1.5207×10^6 by this number, we get 273.7 ,

which is the absolute temperature generally accepted as equivalent to 0°C . Heat absorbed by a gas at constant volume would be at once expressed in dynamical measure by the product of the change in temperature and of two abstract numbers, viz. the ratio of the number of molecules to the standard number, and the ratio of the increase of the total energy of a molecule to that of the energy of translation. Even if some such system should ultimately be adopted, our knowledge of the molecular construction and dynamics of solids and liquids is as yet too imperfect to warrant an attempt to place thermal units on a natural and mechanical basis. Until such a reform can be carried out, I think it would be best to place temperature in a class of *secondary fundamental units*, which, owing either to our ignorance or to our artificial methods of measurement, cannot be expressed in terms of length, mass, and time, and which must therefore be regarded as fundamental to other derived units which depend upon them.

The thermal units could then be tabulated in a more systematic manner than is usual, and the arbitrary character of the system would be emphasized by the symbol employed to represent the dimensions of temperature. If θ be the symbol chosen, if specific heat be taken as a number, and the unit of heat be defined with reference to the unit mass of water, the following results are obtained :—

Quantity of heat	$[\text{M}\theta]$
Mechanical equivalent of heat . . .	$[\text{L}^2 \text{T}^{-2} \theta^{-1}]$
Specific heat	1
Latent heat	$[\theta]$
Thermal capacity	$[\text{M}]$
Coefficients of expansion	$[\theta^{-1}]$
Coefficients of absorption and emission	$[\text{ML}^{-2} \text{T}^{-1}]$
Coefficient of conductivity	$[\text{ML}^{-1} \text{T}^{-1}]$
Entropy	$[\text{M}]$

Gravitation units hardly come within the scope of the present paper, but as they are important in their applications to thermal quantities I may indicate a method of dealing with them.

We must distinguish between the nominal unit of mass in such a system (M) and the real unit ($\text{M}\gamma$), where γ is the abstract number which expresses the acceleration of gravity in terms of the units of length and time. The three fundamental units in such a system are length, time, and force. Mass is a derived unit subject to the condition that $[\text{L} \text{T}^{-2}]$

is a given concrete acceleration. The only practically important cases are those of force, work, power, and the mechanical equivalent of heat, and if we multiply the number expressing any one of these quantities in a gravitation system by (the abstract number) γ , and remember the condition that $\gamma [L T^{-2}]$ is constant, transformations can be effected as usual.

Thus, if J , J' , and J'' be the numerical values of the mechanical equivalent expressed in an absolute, and two gravitational systems, we have

$$J [L^2 T^{-2} \theta^{-1}] = J' \gamma' [L'^2 T'^{-2} \theta'^{-1}] = J'' \gamma'' [L''^2 T''^{-2} \theta''^{-1}].$$

Thus to find the numerical value of J in the C.G.S. system when it is given in the English gravitational system,

$$J = 773.2 \times 32.2 \left(\frac{\text{foot}}{\text{centim.}} \right)^2 \left(\frac{\text{degree Cent.}}{\text{degree Fahr.}} \right) = 4.16 \times 10^7.$$

To find J in the metre-kilogram-second gravitational system, when it is given in the British gravitational system, we reduce the last two of the above expressions by the relation $\gamma' [L' T'^{-2}] = \gamma'' [L'' T''^{-2}]$ to the form

$$J' [L' \theta'^{-1}] = J'' [L'' \theta''^{-1}];$$

so that

$$J = 773.2 \left(\frac{\text{foot}}{\text{metre}} \right) \left(\frac{\text{degree Cent.}}{\text{degree Fahr.}} \right) = 424.$$

To reduce power from an absolute to a gravitational system, or *vice versa*, we have

$$n_1 [M_1 L_1^2 T_1^{-3}] = n_2 [\gamma_2 M_2 L_2^2 T_2^{-3}].$$

Thus to find the number of ergs per second which correspond to one H.P.,

$$n = \frac{33000}{60} \times 32.2 \left(\frac{\text{pound}}{\text{gram}} \right) \left(\frac{\text{foot}}{\text{cm.}} \right)^2 = 7.46 \times 10^9.$$

It is noticeable that although the statement that J is independent of the unit of mass is in general true, it holds good only when the unit of heat and the unit of force are defined with respect to the same unit of mass. In a gravitational system this is not really the case, *e.g.* in the English system the unit of heat is defined with reference to the pound, while the employment of the gravitational measure of force involves a unit of mass = 32.2 lbs. Hence, even when the nominal unit of mass has disappeared, the number which expresses the ratio between the real and nominal units on the gravitational system remains in the expression for a quantity such

as J, which is based upon others which have themselves involved the introduction of mass.

The open admission in the symbols employed that the dimensions of some of the quantities are unknown is more important in the case of the electrical and magnetic than in that of the thermal units.

As Maxwell showed, the independent relations between these quantities are one less than the number required to determine them completely. The electrostatic and electromagnetic systems are therefore deduced from assumptions which are equivalent to suppressing the dimensions of inductive capacity and magnetic permeability respectively. It would, I think, be better to retain in the tables of dimensions symbols which represented these quantities as secondary fundamental units. The student would thus have brought more clearly before him the cause of the difference of the dimensions in the two systems, and the factors necessary to transform from the one system to the other would be indicated.

Thus if, instead of suppressing the dimensions of inductive capacity, we treat it as a secondary fundamental unit of unknown dimensions, we have the equations

$$[\mathfrak{D} \mathfrak{E}] = [M L^{-1} T^{-2}], \quad [\mathfrak{D}] = [K \mathfrak{E}];$$

whence

$$[\mathfrak{D}] = [M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1} K^{\frac{1}{2}}];$$

$$[\mathfrak{E}] = [M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}].$$

In a precisely similar fashion we get

$$[\mathfrak{B}] = [M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}],$$

$$[\mathfrak{H}] = [M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}].$$

By means of these relations all the units can be expressed in terms of M, L, T, K and M, L, T, μ , and the dimensions of M, L, T are those of the electrostatic and electromagnetic systems, according as the first or second mode of expression is adopted.

For since

$$[\mathfrak{D}] = [e L^{-2}], \quad \therefore [e] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} K^{\frac{1}{2}}];$$

and since

$$[\mathfrak{B}] = [m L^{-2}], \quad \therefore [m] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} \mu^{\frac{1}{2}}];$$

and from the dimensions of $[e]$ and $[m]$ those of all the other units can be found.

Maxwell has given a Table in which the dimensions of each of the electrical and magnetic units are expressed in terms of

M, L, T, and of either electrical quantity or the strength of a magnetic pole. My suggestion amounts to nothing more than that we should use a similar list in which the unknown quantities are K and μ instead of e and m . The dimensions in terms of L, M, and T will then be those in the electrostatic and electromagnetic systems, and, instead of suppressing K and μ , we should retain them as representing dimensions which are unknown in terms of the fundamental units.

In cases where it is necessary to indicate whether the quantity represented by a symbol is understood to be expressed in terms of K or μ , we may use the letters s and m subscript to distinguish between them. Thus e_s and e_m are not quantities whose dimensions are fundamentally different, but quantities the dimensions of which are unknown and are expressed in different ways in terms of other unknowns. The letters s and m indicate which of the two methods which are in practical use is employed.

By equating two such expressions for the same quantity, or by using any general relation between any two of the derived quantities, we get a necessary relation between the dimensions of the secondary fundamental units in terms of the primary fundamental units of length, mass, and time.

Thus we have from any one of Maxwell's equations (1) to (12) (p. 241) the general relations

$$[e m] = [M L^2 T^{-1}].$$

If we choose to express e in terms of K and m in terms of μ , the relation still obtains, and thus

$$[e m] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} K^{\frac{1}{2}}] \times [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} \mu^{\frac{1}{2}}] = [M L^2 T^{-1}];$$

$$\therefore [K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = [L T^{-1}].$$

It is obvious that there must be such a relation, for since only one equation is wanting for the complete determination of the dimensions in terms of L, M, and T, we cannot have two independent methods of expressing them.

If, then, we use the letters s and m subscript in the sense above explained,

$$[e_s m_m] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} K^{\frac{1}{2}}] \times [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} \mu^{\frac{1}{2}}],$$

$$[e_s m_s] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} K^{\frac{1}{2}}] \times [M^{\frac{1}{2}} L^{\frac{1}{2}} K^{-\frac{1}{2}}],$$

$$[e_m m_s] = [M^{\frac{1}{2}} L^{\frac{1}{2}} \mu^{-\frac{1}{2}}] \times [M^{\frac{1}{2}} L^{\frac{1}{2}} K^{-\frac{1}{2}}],$$

$$[e_m m_m] = [M^{\frac{1}{2}} L^{\frac{1}{2}} \mu^{-\frac{1}{2}}] \times [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} \mu^{\frac{1}{2}}];$$

and if we employ the relation

$$[K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = [L T^{-1}],$$

we see that the right-hand sides of these expressions are all equal to each other and to $[M L^2 T^{-1}]$.

In this case the secondary fundamental units disappear from the final expression. This would not always be the case for the product of any two units chosen haphazard, but it would always be true that any product or quotient of units could, by the relation

$$[K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = [L T^{-1}],$$

be reduced to the same dimensions in terms of M , L , T , and either K or μ , whether they were or were not originally expressed in terms of one of these two last quantities only.

The method can be applied, not only to express more clearly that the absolute dimensions of the electrostatic and electromagnetic systems are the same, but to transform the numerical expressions for given concrete electrical or magnetic quantities from the one system of units to the other.

In order to transform a length from one scale to another by the relation $n_1[L_1] = n_2[L_2]$ we must know not only n_1 or n_2 but also the numerical value of the ratio L_1/L_2 . In like manner, to transform electrical quantities from a system expressed in terms of K to a system expressed in terms of μ , we must know the numerical value of the ratio

$$K^{-\frac{1}{2}} \mu^{-\frac{1}{2}} / L T^{-1}.$$

If, then, v be the numerical value of the velocity of light expressed in terms of the units L and T , we can transform both dimensionally and numerically by the relation

$$[K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = v [L T^{-1}].$$

Thus, to find the number of C.G.S. electrostatic units in y C.G.S. electromagnetic units of quantity, we have

$$\begin{aligned} x [L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}] &= y \times [L^{\frac{1}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]; \\ \therefore x &= y [L^{-1} T K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] \\ &= y [L^{-1} T] \times v [L T^{-1}] = v y. \end{aligned}$$

To find the number of C.G.S. Electromagnetic Units of Resistance in 20 C.G.S. Electrostatic units.

Working this out from first principles we have, by Maxwell's equations (*loc. cit.*) (2) and (4),

$$[R] = \frac{[E]}{[C]} = [M L^2 T^{-1} e^{-2}];$$

whence, substituting for e in turn in terms of K and μ ,

$$[R] = [L^{-1} T K^{-1}] = [L T^{-1} \mu].$$

Hence

$$x[L T^{-1} \mu] = 20[L^{-1} T K^{-1}].$$

$$\therefore x = 20[L^{-2} T^2 K^{-1} \mu^{-1}].$$

And since

$$[K^{-1} \mu^{-1}] = v^2[L^2 T^{-2}],$$

$$x = 20v^2,$$

which is correct.

In these cases the fundamental units of length, mass, and time, and the medium with respect to which the unit charge and the unit pole are defined, are the same in both systems of units; but the method can be extended to cases in which these conditions do not hold, and which are far more complex than those which are likely to occur in practice.

If the unit charge were defined to be such that two unit charges at a distance of 1 metre in a medium of which the inductive capacity was twice that of air, repelled each other with a force which would, in one second, produce in a centigram a velocity of 1 metre per second, we may find the number of ordinary electrostatic units to which it is equivalent.

We have the general relation

$$x[L_1^{\frac{3}{2}} M_1^{\frac{1}{2}} T_1^{-1} K_1^{\frac{1}{2}}] = 1 \times [L_2^{\frac{3}{2}} M_2^{\frac{1}{2}} T_2^{-1} K_2^{\frac{1}{2}}];$$

$$\therefore x = \left(\frac{\text{metre}}{\text{cm.}}\right)^{\frac{3}{2}} \left(\frac{\text{centigram}}{\text{gram}}\right)^{\frac{1}{2}} \left(\frac{K_2}{K_1}\right)^{\frac{1}{2}} \\ = 10^3 \times 10^{-1} \times \sqrt{2} = 100\sqrt{2}.$$

Thus if it were desired to find the number of C.G.S. electromagnetic units of charge in the unit electrostatic charge above defined, we should proceed as follows:—

$$x[L_1^{\frac{3}{2}} M_1^{\frac{1}{2}} \mu_1^{-\frac{1}{2}}] = 1 \times [L_2^{\frac{3}{2}} M_2^{\frac{1}{2}} T_2^{-1} K_2^{\frac{1}{2}}].$$

But

$$\mu_1^{-\frac{1}{2}} = v_1 K_1^{\frac{1}{2}} L_1 T_1^{-1};$$

$$\therefore xv_1[L_1^{\frac{3}{2}} M_1^{\frac{1}{2}} T_1^{-1} K_1^{\frac{1}{2}}] = 1 \times [L_2^{\frac{3}{2}} M_2^{\frac{1}{2}} T_2^{-1} K_2^{\frac{1}{2}}];$$

where, as before, quantities with 2 subscript refer to the new unit. Of course v_1 must be expressed in terms of centimetres and seconds, as these are the magnitudes of L_1 and T_1 respectively.

$$\therefore x \times 3 \times 10^{10} = 100\sqrt{2}, \quad \therefore x = \frac{\sqrt{2}}{3} \times 10^{-8}.$$

As a final example we may take the following problem, in which the data are chosen merely for the purpose of illustration:—Find the number of C.G.S. electrostatic units of

magnetic induction in 10 electromagnetic units of a system in which the units of length, mass, and time are the metre, centigram, and second respectively, and in which the specific inductive capacity and the refractive index of the standard medium with reference to air are 2 and 1.5 respectively. Assume that $K^{-\frac{1}{2}}\mu^{-\frac{1}{2}}$ is proportional to the velocity of light in the medium.

We have already seen that

$$[\mathfrak{B}] = [M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}] = [M^{\frac{1}{2}} L^{-\frac{3}{2}} K^{-\frac{1}{2}}].$$

Hence if a subscript refers to air,

$$x [(\text{gram})^{\frac{1}{2}} (\text{cm.})^{-\frac{3}{2}} K_a^{-\frac{1}{2}}] \\ = 10 [(\text{centig.})^{\frac{1}{2}} (\text{metre})^{-\frac{3}{2}} (\text{sec.})^{-1} \mu^{\frac{1}{2}}].$$

We may now make the substitution

$$K_a^{-\frac{1}{2}} = 3 \times 10^{10} \mu_a^{\frac{1}{2}} (\text{cm.}) (\text{sec.})^{-1};$$

or else calculating the velocity of light in the medium from the refractive index, we may write

$$\mu^{\frac{1}{2}} = \frac{1.5}{3 \times 10^8} K^{-\frac{1}{2}} (\text{metre})^{-1} (\text{sec.}).$$

By the first substitution we get

$$x \times 3 \times 10^{10} [(\text{gram})^{\frac{1}{2}} (\text{cm.})^{-\frac{3}{2}} (\text{sec.})^{-1} \mu_a^{\frac{1}{2}}] \\ = 10 [(\text{centig.})^{\frac{1}{2}} (\text{metre})^{-\frac{3}{2}} (\text{sec.})^{-1} \mu^{\frac{1}{2}}];$$

$$\therefore x = \frac{10^{-9}}{3} \left(\frac{1}{10}\right) \times \left(\frac{1}{10}\right) \times \left(\frac{\mu}{\mu_a}\right)^{\frac{1}{2}} \\ = \frac{1}{3} \times 10^{-11} \times 1.5 \left(\frac{K_a}{K}\right)^{\frac{1}{2}} = \frac{1}{2\sqrt{2}} \times 10^{-11}.$$

By the second substitution,

$$x [(\text{gram})^{\frac{1}{2}} (\text{cm.})^{-\frac{3}{2}} K_a^{-\frac{1}{2}}] \\ = 10 \times \frac{1.5}{3 \times 10^8} [(\text{centig.})^{\frac{1}{2}} (\text{metre})^{-\frac{3}{2}} K^{-\frac{1}{2}}]; \\ \therefore x = \frac{1}{2 \times 10^7} \left(\frac{1}{10}\right) \times \left(\frac{1}{100}\right) \left(\frac{K_a}{K}\right)^{\frac{1}{2}} = \frac{1}{2\sqrt{2}} \times 10^{-11};$$

which is the same result as before.

In such a problem the amount of time and thought saved by the method of dimensions extended to secondary fundamental units is considerable.

I think that these examples are sufficient to justify the change in the method of exhibiting the dimensions of electrical quantities which I advocate, apart from the great theoretical advantage of being able to write electrostatic and

electromagnetic quantities as being of the same absolute dimensions.

I therefore give a Table in which the powers in which K and μ enter into the various electrical and magnetic quantities are shown.

If we put $K=1$ we get the ordinary electrostatic, if $\mu=1$ the ordinary electromagnetic dimensions.

Table of Dimensions.

	Symbol.	Dimensions in terms of	
		L, M, T, and K.	L, M, T, and μ .
Quantity of electricity . . .	e	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]$
Electric potential . . .	E	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Quantity of magnetism . . .	$\left\{ \begin{matrix} m \\ p \end{matrix} \right\}$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Electrokinetic momentum of a circuit			
Electric current	$\left\{ \begin{matrix} C \\ \Omega \end{matrix} \right\}$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Magnetic potential	$\left\{ \begin{matrix} \Omega \\ \mathfrak{D} \end{matrix} \right\}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]$
Electric displacement	$\left\{ \begin{matrix} \mathfrak{C} \\ \mathfrak{B} \end{matrix} \right\}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2} \mu^{\frac{1}{2}}]$
Electromotive force at a point		$[L^{-\frac{3}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Magnetic induction	\mathfrak{H}	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Magnetic force	$\left\{ \begin{matrix} \mathfrak{C} \\ \mathfrak{A} \end{matrix} \right\}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Strength of current at a point		$[L^{-\frac{3}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Vector potential			
Capacity	$\left[\frac{e}{E} \right] = [q]$	[LK]	$[T^2 L^{-1} \mu^{-1}]$
Coefficient of self-induction	$\left[\frac{p}{C} \right] = [L]$	$[L^{-1} T^2 K^{-1}]$	$[L\mu]$
Specific inductive capacity	$\left[\frac{\mathfrak{D}}{\mathfrak{C}} \right] = [K]$	[K]	$[L^{-2} T^2 \mu^{-1}]$
Magnetic inductive capacity	$\left[\frac{\mathfrak{B}}{\mathfrak{H}} \right] = [\mu]$	$[L^{-2} T^2 K^{-1}]$	$[\mu]$
Resistance	$\left[\frac{E}{C} \right] = [R]$	$[L^{-1} T K^{-1}]$	$[L T^{-1} \mu]$
Specific resistance	$\left[\frac{\mathfrak{C}}{\mathfrak{A}} \right] = [r]$	$[T K^{-1}]$	$[L^2 T^{-1} \mu]$

To these we may add certain units in which both electrical and thermal quantities occur.

Thus, using the relations :—

Thermoelectric height \times temperature = E.M.F.,

or Specific heat of electricity \times temperature = E.M.F.,

$\Pi \times$ current \times time = heat,

or $J\Pi$ = E.M.F.,

we get the following results :—

	Symbol.		
Thermoelectric height . .	} kt	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}} \theta^{-1}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2} \mu^{\frac{1}{2}} \theta^{-1}]$
Specific heat of electricity			
Coefficient of Peltier effect	Π	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} T K^{-\frac{1}{2}} \theta]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} \mu^{\frac{1}{2}} \theta]$
Geometrical capacity . .	$q' = \frac{q}{K}$	$[L]$	$[L]$

In conclusion, then, I think it would be well to introduce symbols for what I have called secondary fundamental units instead of suppressing their dimensions, for the following reasons :—

(1) We thereby generalize the method of determining by dimensional equations the relation between a change in the magnitude of the units and in the number of which a given quantity is expressed.

(2) In the case of the electrical and magnetic units the method indicates clearly the cause of the difference between the dimensions in terms of M, L, and T of the same quantity in the two systems.

(3) It indicates that the dimensions of different quantities in the same system, which are apparently the same with respect to M, L, T, really are or may be different, as their dimensions with respect to K or μ are different ;

$$e.g. [\mathfrak{D}] = [L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$$

$$[\mathfrak{E}] = [L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}].$$

(4) It suggests an explanation of the artificial and unintelligible character of the electric and magnetic units. If the dimensions of K and μ were known, they would probably be simplified.

(5) Lastly, I think the symbols are thus made to express the limits of our knowledge and ignorance on the subject more exactly than if we arbitrarily assume that some one of the quantities involved is an abstract number.

XIV. *On the Definition of the Terms "Energy" and "Work."*
By SIMON NEWCOMB*.

THE accepted definitions of the terms "kinetic energy" and "work" are substantially these:—

The kinetic energy of a moving body is measured by half the product of its mass into the square of its velocity.

The work done by a force acting upon a body is the product of the intensity of the force into the motion of the body in the direction in which the force acts.

It will be noticed that the terms "velocity" and "motion" are here used as if their measures were absolute. But it is universally understood that motion, and therefore velocity, are relative terms; that no body considered in itself can be said to be either in motion or at rest, because motion and rest can be defined only as the motion or rest of one body relatively to some other body, real or imaginary. It follows that we may assign to any one body any arbitrary motion we choose.

Such being the case, it would seem to follow from the above definitions that the energy of a moving body considered by itself is an entirely arbitrary quantity; that the same is true of the work done upon a body; and that, when we consider the kinetic energy of a system, its amount will depend upon the origin to which we refer the motion of the system. The value of the kinetic energy will be a minimum when the motion which we assign to the centre of gravity of the system is zero, and may be greater than this minimum by an amount which will depend upon the motion of the centre of gravity relative to the point of reference. Not only is the work done by a force acting on a moving body arbitrary for the same reason, but we cannot assign any value to the work necessary to change one given motion A into another given motion B, though the relation of B to A be completely given.

The question now arises, In what form are we to define these seemingly arbitrary quantities so as to make their amounts definite? This requires an improved statement of Newton's third law. As usually formulated, this law implies, but does not completely express, a universal condition of the action of every mechanical force with which we are acquainted, namely:—

No force ever acts except between bodies; and every force so acting on a body A is a mutual action between that body and some other body B, such that the actions on the two bodies are equal and opposite.

* Communicated by the Author.

The lack of explicitness in the current statements of this law is noteworthy. The first clause in Newton's statement of the law, "*Actioni contrariam semper et æqualem esse reactionem*," does indeed imply, but does not explicitly state, that the reaction is exerted upon a body from which the action must emanate. In all the authorities which I have noticed even the implication is wanting, the law being stated in the hypothetical form—when one body acts upon another, the other acts upon it, &c. This statement can scarcely be said even to imply that there is never any action upon one body except such as emanates from another body.

We may now give precision to the definition of work by putting it in this form :—

The work done by a force is the product of the intensity of the force into the amount by which the two material points between which it acts approach to or recede from each other ; the work being positive when the approach or recession is in the direction of the force, negative in the opposite case.

The quantity of work thus defined is independent of the point to which we refer the motion of the two bodies, since only the relative motion is involved, and since two bodies must, and two only can, come into play in the case of any one force.

The case is different with energy. It cannot be defined in terms of the relative motion even of two bodies ; because, in this simplest case, if the masses are different the energy will be greater when we refer the motion to the body of smaller mass. Hence, in defining the quantity of energy, we must always implicitly assume some point of reference ; and this point being arbitrary, the total energy either of a body or of a system is necessarily an arbitrary quantity.

Moreover, whether the work is done upon a body or by a body in changing its motion from one state to another, depends altogether upon the mass and motion of the foreign body from which the action emanates. The law of the conservation of energy assumes that we refer the motions of all the bodies whose energy is considered, to some foreign body of infinite mass, from which emanate the forces which give motion to the system. The energy of the system is then equal to the work it would do by being brought to a state of rest by forces acting between its own parts and the body of reference. In the familiar problems of kinetic energy we take as our body of reference a point on the surface of the moving earth, and then regard the energy of the moving body as the work done or to be done upon it by reaction against the earth, or some body fixed with reference to the earth.

I cannot but think that sound philosophy would be promoted could these limitations of the doctrine of the conservation of energy be made clear to those philosophers who see in the doctrine only a special case of a general law of mind and matter.

XV. *Combined Effects of Torsion and Longitudinal Stress on the Magnetization of Nickel.* By H. NAGAOKA, *Rigakushi, of the Imperial University, Japan.* With a Note by J. T. BOTTOMLEY, *F.R.S.*, and A. TANAKADATÉ, *Rigakushi* *.

[Plates II.-V.]

THE effect of torsion in altering the induced magnetism of iron has long engaged the attention of many physicists. Among the experimenters in this field of research may be named Wertheim, Wiedemann, Thomson, and Tomlinson (whose latest work on such subjects I have not yet seen). The experiments of Wiedemann were made by twisting and untwisting the wire, which was placed horizontally, in a magnetizing solenoid, till the changes of magnetism became cyclic. But it was Thomson who first investigated the effect of torsion on the magnetism of iron, the wire being at the same time subject to definite longitudinal stresses. In his experiments the soft iron wire was placed vertically in the earth's field. No one seems to have made similar experiments in different magnetizing fields. Scanty though this kind of investigation has been for iron, it is still more scanty for nickel. Indeed, so far as I am aware, the effect of torsion on the magnetism of nickel wire under various longitudinal stresses has not hitherto been investigated. This accordingly was the problem I resolved to attack ; and the results that have been obtained will, I believe, be found to contain distinct novelties.

It is well known from the results of various experimenters that, by the application of longitudinal stress, the magnetism of iron increases up to a certain critical load, while that of nickel always diminishes. Thus we should naturally expect that the effect of torsional stress on nickel will be opposite to that on iron. In fact, this is exactly reproduced in one of Wiedemann's experiments †, the curve obtained being just the reverse of one given by Thomson ‡. But Wiedemann's result was obtained by simply clamping the wire in a horizontal position, so that the wire was subject to a weak longi-

* Communicated by Sir William Thomson. The paper of Mr. Nagaoka was originally published in the *Journal of the College of Science, Imperial University, Japan*, 1888.

† Wiedemann's *Annalen*, Bd. xxvi. S. 376 (1886).

‡ Philosophical Transactions, 1879, p. 72.

tudinal stress only. On this account the combined effect of pull and torsion on the magnetism of nickel was still a matter to be determined.

In the following experiments I have examined these points, and have found that the longitudinal stress produced a singular effect. For weak stresses the change of magnetism came out as was to be expected, but when the load exceeds a certain limit this is no longer the case. The changes of magnetism become gradually altered, and beyond a critical value of the longitudinal stress one end of the nickel wire acquires the two opposite kinds of magnetism during the torsion and detorsion, notwithstanding the absolute constancy of the magnetizing force both in direction and magnitude. This critical value of the load seems to vary with the strength of the magnetizing field, becoming greater as the field is increased. All these points will be described in the following pages.

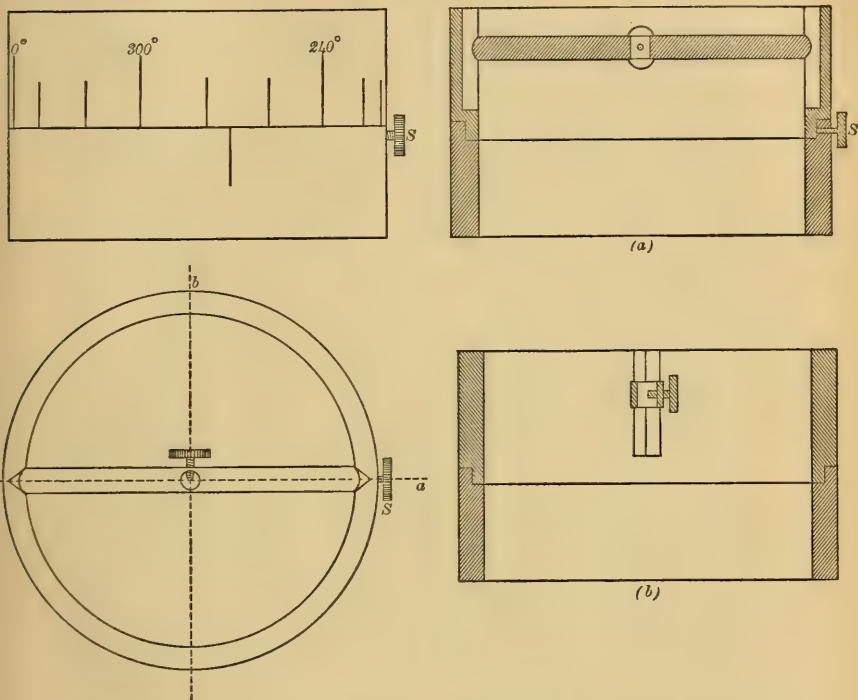
I must here express my thanks to Dr. C. G. Knott for his kind suggestions during the course of experiments.

The intensity of magnetization was measured by a direct magnetometric method. The magnetometer consisted of a small mirror hung by a spider-thread 11 centim. long. This was geometrically fixed in position on a wooden plank according to Thomson's method of the hole slot, plane. Leveling was effected by three base-screws. In front of the magnetometer a lamp was placed, and the image of the slit was reflected on a circular scale. Its radius was 1 metre, and it was so placed that the magnetometer was just at its centre. The wire to be examined was set vertically due east of the magnetometer. The upper end of the wire was level with the centre of the magnetometer-mirror. To each end of the wire a short stout brass wire was brazed. The lower of these was bent into a hook, so that a pan holding the weight could be hung from it. The upper one was riveted to a strong brass rod projecting from the middle of the side of a table, which rested on stone piers. The nickel wire* was surrounded by a magnetizing coil 45 centim. long. The resistance of the coil was 19.6 ohms, and the strength of the field for a current of one ampere was 138.4 C.G.S. units. The magnetizing current was sent from 12 Daniell cells, and its strength was adjusted by means of a liquid slide and measured by a tangent galvanometer.

The twisting-apparatus is shown in the subjoined cut.

* This wire contained 1.7 per cent. of iron, besides small quantities of carbon as impurities.

It consisted of two hollow cylinders of 2.5 centim. radius. The lower cylinder was fixed to a tripod stand, while the upper one fitted into it, and was movable. The latter was graduated on its external cylindrical surface at intervals of 20° , and by means of a pointer which was cut on the corresponding surface of the fixed cylinder the amount of twist was



easily read off. The screw, S, attached to the lower cylinder serves to fix the upper one at any desired angle of twist. On the inner surface of the movable cylinder two vertical V-grooves were cut opposite each other. On these the two ends of the thick brass diametral rod were made to slide. This rod had a small hole at its centre which was just large enough to allow the passage of the brass wire attached to the lower end of the nickel wire. In order to secure the axial position of this hole with reference to the twisting-cylinder, the rod was fixed between the V's and bored on a lathe by turning it together with the cylinder. A small clamping-screw served to pin the wire fast against the side of the rod, so that the wire and cylinder rotated together.

It might at first sight appear that this arrangement might prevent the longitudinal stress being applied uniformly for various angles of the twist, because of the friction of the rod against the V's. To test this point the wire was fixed by the screw and the longitudinal pull applied by known loads hung on below. The upper end of the wire was fastened to a spring-balance, by means of which any variations of stress could at once be detected. With a given load, the wire was twisted through various angles; but scarcely any sensible variation of longitudinal stress was indicated.

The magnetic experiments were conducted in the following manner:—At first a constant current was made to pass through the magnetizing coil and the magnetometer-zero was determined. The wire was then placed in position, and was first twisted through 180° in what we call the positive direction; although it may be stated once for all that the positive direction means the *first chosen* direction, whether that is, so to speak, with the magnetizing current or against it. Then after a complete revolution in the opposite direction, it was brought back to its original position. This process was repeated till the changes became nearly cyclic. Then at every successive 20° of twist, the deflexion of the magnetometer-magnet as given by the scale-reading was taken and noted, the reading being observed by means of a telescope. Each complete set of experiments was made in a constant magnetic field, while the wire was subjected to gradually-increasing longitudinal stresses. The results thus obtained are given in C.G.S. electromagnetic units, though such reduction is not necessary in experiments of this kind. The observed values of the intensity \mathfrak{J} for successive 60° of twist are given in the Appendix. In the figures showing the changes of magnetization, the abscissæ denote the amount of twist, while the ordinates represent the intensity of magnetization \mathfrak{J} .

The first experiment was made with a nickel wire 40 centim. long, and 1 millim. thick. The wire was deprived of its initial magnetism by heating it red-hot. It was then placed vertically, and so came under the influence of a magnetic field of $\cdot 34$ C.G.S. units. With a steady load of $\cdot 64$ kilog. the wire was subjected to repeated twisting and untwisting. After six such operations, the changes became nearly cyclic, and the following readings of deflexions were taken at the seventh cycle:—

Twist (positive).	Readings.	Twist (negative).	Readings.
0	24.2	0	3.4
20	35.8	20	-0.2
40	45.5	40	-2.0
60	55.8	60	-2.4
80	64.4	80	-2.6
100	71.0	100	-2.4
120	76.3	120	-1.9
140	79.8	140	-1.2
160	82.2	160	-0.5
180	83.8	180	0.0
160	81.9	160	0.0
140	78.8	140	-0.2
120	73.5	120	-0.3
100	65.2	100	-1.0
80	51.9	80	-1.0
60	35.9	60	1.9
40	22.0	40	7.8
20	10.8	20	17.3
0	3.4	0	28.7

The above readings, reduced to absolute units, are shown plotted in fig. 1 (Pl. II.). The amount of load per square centimetre was 82 kilograms, and the twist of 180° corresponded to that of .0785 radian per centimetre.

Examining the figure, we see that the first effect of twisting is to increase the magnetization. The rate of increase is rapid at first, but gradually falls off as the magnetization attains its greatest value at the maximum twist. During the process of untwisting the magnetization diminishes more rapidly than it increased during twisting, so that for every position of twist the magnetization during twisting is greater than the magnetization during untwisting. The diminution of magnetization goes on even after the wire has passed the original position from which the twisting was begun until the apparent magnetization is at length reduced to zero. This happens very soon after the original position of the untwisted wire has been reached, as the process of untwisting is continued as twisting in the negative direction. But now as this negative twisting is continued, the *polarity of the wire changes sign*, a very striking fact indeed. As the twisting is continued on towards -180° , this negative magnetization passes through an arithmetical maximum, becoming finally almost zero. As the wire is being brought back to the position from which it started, the magnetization gradually recovers nearly its original value, as shown in the figure.

Now, reasoning from analogy, we should expect to obtain

by such twisting and untwisting a curve of the form given in Pl. II. fig. 5, since the behaviour of nickel with regard to the effect of stress in magnetization seems to be just opposite to that of iron (see Sir W. Thomson's figures for iron, *Philosophical Transactions*, 1879). But in this experiment the curve of magnetization seems to have no resemblance at all to any figured by Sir W. Thomson; and then there is the very curious fact that the magnetization of nickel in a steady field can be made to change sign by twisting. It first occurred to me that this very extraordinary result must be due to some defect in the arrangement, but careful examination discovered no flaw. The question naturally suggests itself, Was this phenomenon a function of the load as well as of the twist? Hence, as a next step, the weight was increased by $\cdot 5$ kilog., and the experiment was performed in the usual manner.

The result is shown in fig. 2. Here the effect is quite similar to the former, the differences being only differences of detail. The march of magnetization with positive twisting is sensibly the same as in the former case; but during negative twisting the opposite magnetization has increased to more than ten times its amount in the first experiment. However, the rate of recovery has very much diminished, and even after a twist of 180° the magnetization is far from reaching the former value.

The same experiment was then performed with the load increased to $5\cdot 14$ kilogs. The result is shown in fig. 3. There again we find the opposite magnetization still more increased. Indeed, the two kinds of magnetization do not differ much in intensity at the two extreme twists, although the initial or positive magnetization is somewhat predominant. In the two former experiments there was a distinct tendency towards recovery of positive magnetization as the wire was twisted more and more in the negative direction. Here, however, no such tendency shows itself except in the diminished rate of growth of negative magnetization.

Still increasing the load, we see that the curve (fig. 4) becomes nearly symmetrical with respect both to the line of zero magnetization, and the line of zero twisting. In this case we find further that the range of the change of magnetization has considerably diminished. The slight excess of the initial magnetization over the other still shows itself, a fact which is probably to be referred to the direction of the magnetizing force.

The general conclusion from these experiments is that in a weak field of $\cdot 34$ units, the increase of load makes the manner of change of magnetization in nickel under the influence of

cyclic twisting depart more and more from any slight resemblance which at small loads it seemed to bear to the manner of change for iron. For still smaller loads, then, it might be possible to obtain the magnetization-curve just opposite to that of iron.

In the second series of experiments the strength of the field was raised to 2·47 units. The load at first applied was only the weight of the brass wire attached to the lower end of the nickel wire, and a brass rod which gripped the wire during the process of twisting. This load was ·02 kilog., that is, a tension of 2·6 kilogs. weight per sq. centim. With this amount of longitudinal stress, the successive twistings and untwistings were performed seven times, and the following readings of deflexion were taken :—

Twist (positive).	Readings.	Twist (negative).	Readings.
0	148·7	0	148·0
20	166·4	20	167·4
40	181·5	40	183·5
60	193·2	60	195·3
80	201·1	80	202·9
100	205·9	100	208·0
120	209·0	120	210·0
140	210·3	140	211·1
160	210·9	160	211·5
180	211·0	180	211·6
160	207·4	160	207·9
140	201·8	140	202·0
120	192·5	120	193·4
100	177·2	100	178·3
80	157·3	80	157·6
60	136·6	60	137·5
40	126·9	40	127·0
20	133·0	20	132·2
0	148·0	0	148·0

The curve thus obtained (see fig. 5) is nearly perfectly symmetrical with respect to the line of zero twisting. Also, the magnetization remains positive throughout the whole cycle. It is, moreover, interesting to observe that the curve is exactly the reverse of that of iron as obtained by Thomson. This shows that the behaviour of nickel twisted in a magnetic field under feeble loads is opposite to that of iron.

When the stress was increased to 145 kilogs. weight per sq. centim., the magnetization-curve lost its symmetry and became as shown in fig. 6. The intensity of the magnetization became greatly diminished, but still remained positive throughout the whole cycle of operations. The features to be noted

are that, although suggestive of the symmetrical form given in fig. 5, the curve is now distinctly one-sided with respect to the line of zero twisting, and indicates much greater magnetization for positive than for negative twists. These peculiarities are still more pronounced when the stress is increased to 209 kilogs., as shown in fig. 7.

This experiment is of special interest, illustrating as it does, the manner in which the magnetization-cycle changes character just as the extraordinary phenomenon of change of sign is about to show itself.

As usual, the first effect of twisting the wire is to increase the intensity of magnetization, while the effect of untwisting it is to decrease it. Then, as the untwisting is continued as negative twisting, the magnetization tends to recover its former value. But for the maximum negative twist of two right angles, the magnetization does not nearly recover its former value. Thus it is evident that in such a strength of field the increase of longitudinal stress tends to make the increase of magnetization during negative twisting gradually less and less, until finally for a certain load the increase does not take place at all for the particular range of twist. This is shown in fig. 8, which is the curve for a tension of 782 kilogs. weight per sq. centim.

A study of these four curves (figs. 5, 6, 7, 8) shows the character of the changes wrought in the cycles as the load is increased. The symmetry is first lost, the negative loop becoming smaller and smaller. Then, as shown in fig. 7, it ceases to be a loop, the course of the return curve from greatest negative twist lying above the other, and never cutting it. Then, as in fig. 8, the *upward* course of the curve on the negative side of zero twist vanishes away altogether; while at the same time the phenomenon of reversal of magnetic polarity shows itself. Thus the double-looped curve for low tensions passes gradually into a single-looped curve as the tension is increased. And after this single-looped curve is obtained, the phenomenon of reversed polarity begins to appear. The passage from the double-looped to the single-looped curve betokens a peculiar alteration in the lagging-effect in nickel—an alteration which has no analogue in the case of iron.

To study more carefully the law of *hysteresis* in nickel—to use Professor Ewing's word—the experiments were repeated in stronger fields.

Figs. 9, 10, 11, 12 (Pl. III.) show the march of events in a field of 4.94 units, the tensions increasing from 400. For smaller tensions the curves are of the approximately symmetrical form shown already in fig. 5, and do not call for special

remark. Here, then, we see how the already diminished loop for negative twisting, as shown in fig. 9, has vanished altogether in fig. 10. At the same time, the curve begins to cross the lines of zero magnetization into the negative region. Fig. 11 is a further development in the same direction. In both of these curves (10 and 11), the quantity $\frac{d\mathfrak{J}}{d\tau}$, the rate of change of magnetization with twist, still changes sign at particular twists. The negative *tail*—as it might be called—so evident in fig. 10, has disappeared in fig. 11; while at the same time the negative magnetization has numerically increased. But now passing to the higher load (see fig. 12), we see that the various changes discussed above have their final end in a simple single-looped curve, with a large portion in the region of negative magnetization, and with no true minimum-points for \mathfrak{J} . The twist for which, in the first three cases, the value of $\frac{d\mathfrak{J}}{d\tau}$ changes sign, works towards the left as the load is increased. Thus :—

$$\begin{array}{l} \text{For } W=400, \quad d\mathfrak{J}/d\tau=0 \text{ at } + 8^{\circ}. \\ \quad \quad \quad W=527, \quad \quad \quad \text{,,} =0 \text{ at } -20^{\circ}. \\ \quad \quad \quad W=655, \quad \quad \quad \text{,,} =0 \text{ at } -60^{\circ}. \end{array}$$

For $W=1910$, we may regard this critical twist as being too great to be included in the range of greatest twist applied.

It was remarked, while discussing the experiment made in the earth's vertical field, that the ratio of the two opposite magnetizations gradually tends to unity as the load is increased; but this does not seem to be generally the case. There is a certain limit beyond which there is an opposite tendency. The following calculations show that this must be the case:—Let $+\mathfrak{J}$ and $-\mathfrak{J}$ be the greatest magnetization during positive and negative twists respectively, then in the field = 4.94 units, and

$$\begin{array}{l} W = 655 ; \quad +\mathfrak{J}=188.6, \quad -\mathfrak{J}= 49.9, \quad +\mathfrak{J}/-\mathfrak{J}=3.78 ; \\ W = 910 ; \quad +\mathfrak{J}=152.1, \quad -\mathfrak{J}=151.3, \quad +\mathfrak{J}/-\mathfrak{J}=1.00 ; \\ W = 1270 ; \quad +\mathfrak{J}=119.3, \quad -\mathfrak{J}=113.1, \quad +\mathfrak{J}/-\mathfrak{J}=1.05 ; \\ W = 1910 ; \quad +\mathfrak{J}= 86.3, \quad -\mathfrak{J}= 77.4, \quad +\mathfrak{J}/-\mathfrak{J}=1.11. \end{array}$$

The following experiments in field = 6.71, show how this ratio depends on the strength of the field. The changes which the magnetization-curve undergoes while it is changing its sign are quite analogous to the preceding two cases, as a glance at figs. 13, 14, and 15 will show. From figs. 15, 16 (Pl. III.), 17 (Pl. IV.), however, we see how the opposite magnetization becomes smaller as we increase the field. The ratio $+\mathfrak{J}/(-\mathfrak{J})$ is always a very large quantity and has a minimum value for

a particular load. For loads greater than this particular load the negative magnetization decreases, and at last completely vanishes. Thus, for stress $W=1770$ kilogs., there is no opposite magnetization therein agreeing with the curves for loads smaller than that for which the negative magnetization first appears. But there is the difference that the curve is a single loop in which $d\mathfrak{J}/d\tau$ changes sign during negative twisting and untwisting.

Taking into account all the series of experiments, we see that the greatest value in each series of the ratio $+\mathfrak{J}/-\mathfrak{J}$ tends to increase as the field is increased. The strength of field in which these last experiments were tried seems to be about the critical value for which we can get the two transitions of magnetization—namely the change of sign for particular load and the vanishing of this change for a higher load.

The peculiarities which have just been the subject of discussion do not, however, persist at all strengths of fields. At still higher fields a different order of things comes in. Take, for example, the next series of experiments with a field of 8.06, as shown in figs. 18, 19, 20, 21, 22. First of all, for the lower tensions, the two-looped curve is more symmetrical than it can be obtained at lower fields (see fig. 18). In fig. 19, for a tension of 782, a symmetry begins to show itself; but the diminution, instead of taking place in the left-hand or negative loop, takes place in the right-hand or positive loop. Figs. 20, 21 show the gradual vanishing away of this right-hand loop as the load is increased. Also, exactly as in the former sets of experiments, the curve dips below the zero-magnetization line, as the right-hand portion loses its loop-character. Now if we were to compare fig. 21 with figs. 17, 12, and 8, 2, and take no account of the intermediate links of development, we should at once regard them as being of essentially opposite character. In the four earlier cases positive twist increased the magnetization and negative twist diminished it; but in the present case the effects are exactly opposite. In the same way fig. 20 presents features quite opposite to those presented by figs. 1, 7, 11, and 16. However, just as in the former sets of experiments, the twist at which $d\mathfrak{J}/d\tau$ vanishes was shown to shift gradually to the negative side as the load was increased; so in the present case, the twist at which $d\mathfrak{J}/d\tau$ vanishes shifts gradually to the right as the load is increased. These very interesting reversals of effects clearly depend on the strength of the field. Then, again, the particular strength of field at which the reversal of these effects begins to show itself seems to be connected with the fact already discussed, that for a particular field the ratio

$+J/(-J)$ becomes infinite. In other words, when the strength of the field is such that, under sufficient loading, the reversal of polarity vanishes away, this seems to be the signal for the new set of conditions to appear. Up to this critical strength of field, it is the left-hand loop in the typical symmetrical curve that gradually diminishes under loading. But for strengths of fields higher than this critical value, it is the right-hand loop that disappears when the load is great enough. This reversal of effects also seems to be accompanied by a lingering of the magnetization near the zero (see figures 18 and 19).

It is not necessary to discuss in detail other combinations of field and stress which were experimented upon. There are certain minor differences depending on strength of the field; but the principal features for fields higher than 8 are the same. The essential characteristics can be gathered from figures, brief explanations of which I shall content myself with giving.

Figs. 22-23 (Pl. IV.).—These illustrate the changes of magnetization in field 11·9.

Figs. 24-27 (Pl. IV.).—These were obtained in field 13·85. The curious transition-curve, fig. 30, Pl. V., is specially worthy of note.

Figs. 28-31 (Pl. V.).—These show the gradual changes of magnetization for field 15·78.

Figs. 32-34 were obtained in field 23·50.

Figs. 35-40 were obtained in field 33·54.

This last was the highest strength of the field at which it was possible to notice the changes of magnetization. For the stronger the field the higher is the load necessary to bring out the curious changes. The critical load for fields higher than 33·54 is greater than the tenacity of the wire.

There is one other point that calls for remark. The range of the change of magnetization under feeble stresses begins gradually to diminish after a certain strength has been reached, so that for a field of 20 or 30, the change of magnetization by twisting becomes almost inappreciable. This can be accounted for by the fact that nickel wire, whether in the normal or twisted condition, behaves practically the same as regards magnetization in higher fields. Indeed, nickel is more easily saturated than iron, so that when $J=20$ or 30, it is already far beyond the saturation-point. Consequently the differences in the susceptibilities of nickel in the normal and twisted conditions, to which this alteration of magnetic intensities must be ascribed, become less and less marked as the strength of the field is increased. Many of the features of magnetization-curves are quite simply accounted for by this consideration.

It is very difficult to determine the exact loading at which the reversed phenomenon of reverse polarity makes its appearance in various fields. Assuming, however, that within small ranges of load, the decrease in the intensities of magnetization is proportional to the amount of loading, I obtained the following values for the stress which must be applied so as to effect the reversal of polarity of the particular nickel wire placed in various fields :—

For $\mathfrak{H} = \cdot 34$, $W = 77$ kilogs. centim.²

$\mathfrak{H} = 2\cdot 47$, $W = 217$ " "

$\mathfrak{H} = 4\cdot 94$, $W = 421$ " "

$\mathfrak{H} = 6\cdot 71$, $W = 654$ " "

$\mathfrak{H} = 8\cdot 06$, $W = 783$ " "

$\mathfrak{H} = 11\cdot 92$, $W = 1120$ " "

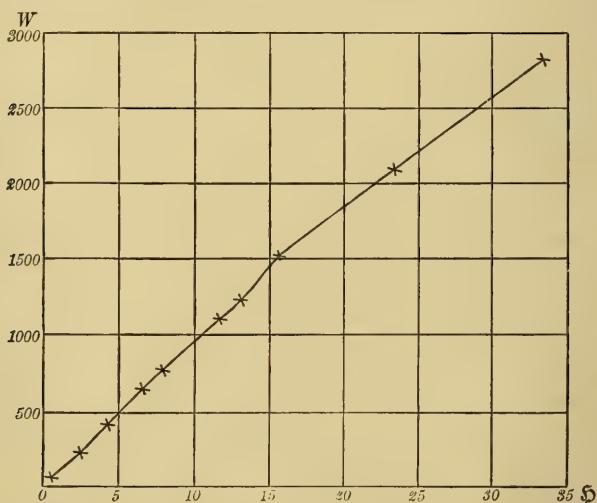
$\mathfrak{H} = 13\cdot 85$, $W = 1220$ " "

$\mathfrak{H} = 15\cdot 78$, $W = 1530$ " "

$\mathfrak{H} = 23\cdot 50$, $W = 2110$ " "

$\mathfrak{H} = 33\cdot 54$, $W = 2830$ " "

Plotting the curve, with \mathfrak{H} for abscissa and W for ordinate, we get the annexed figure :—



This seems to show that, for moderate strengths of field, the load at which the wire begins to show reversed polarity is nearly directly proportional to the strength of the field. For very weak and strong fields this rule does not seem to hold. It must be remembered, of course, that all these peculiarities are for one particular twist only, and that it is possible that quite a different series of effects might exist for other twists.

The general results of these experiments may be thus summarized.

In all magnetic fields with moderate loading, the effect of twisting nickel wire is to increase the magnetization. But the increase depends on the strength of the field as well as the longitudinal stress applied. If the field is weak and the longitudinal stress sufficiently great, the magnetization increases in one direction of twist and decreases in the other. Eventually, for a particular stress which is approximately proportional to the field, the wire begins to show opposite polarity; and the cyclic curve of magnetization passes gradually from a two-looped to a single-looped form. For stronger fields similar effects exist. But in fields higher than a critical value the increase and decrease of magnetization take place for reversed directions of twist, and at the same time the course of the curve becomes reversed.

In a recent paper ("Magnetische Untersuchungen," Wiedemann's *Annalen*, March 1886) Professor Wiedemann has described certain experiments on the combined effects of magnetization and twist in iron and nickel. He does not seem, however, to have investigated the effect of longitudinal stress in conjunction with these. His chief aim seems to have been to adduce facts in support of his theory of frictionally rotated molecules. Some of the results above described may be expressed in terms of his theory. Thus, when the external magnetizing force is great, the magnetic molecules will be held in position more strongly, and consequently the change of magnetization due to twisting will be diminished. This agrees with experiment. But, again, we saw that by sufficiently loading the wire we could bring the apparent magnetization down to zero, and eventually reverse its sign by mere twisting. Now if this be due to the frictional rotation of molecules, the molecules must, notwithstanding the directive force of 30 units, be rotated through more than a right angle from their first position, while the amount of mechanical twist amounts only to .079 radian per centim. in each direction. Admit it to be so; what effect then may we expect increased loading to produce on the rotation of the molecules? The magnetic molecules in strong fields are acted on only by a greater directive force, and consequently they must tend to remain more in the direction of the magnetizing force; but why they should assume nearly the position of magnetic neutrality when they are subjected to sufficient longitudinal stress is a question which every supporter of the theory of frictionally rotated molecules is bound to answer.

APPENDIX.

$\delta=0.34.$					$\delta=2.47.$					$\delta=4.94.$				
$\tau.$	$\zeta.$ W=82. (Fig. 1.)	$\zeta.$ W=273. (Fig. 2.)	$\zeta.$ W=655. (Fig. 3.)	$\zeta.$ W=1280. (Fig. 4.)	$\zeta.$ W=2.5. (Fig. 5.)	$\zeta.$ W=145. (Fig. 6.)	$\zeta.$ W=273. (Fig. 7.)	$\zeta.$ W=782. (Fig. 8.)	$\zeta.$ W=82. (Fig. 9.)	$\zeta.$ W=400. (Fig. 10.)	$\zeta.$ W=655. (Fig. 11.)	$\zeta.$ W=1270. (Fig. 12.)		
0	32	54	51	33	195	59	112	109	207	36	115	49		
+ 60	73	99	87	61	253	155	196	159	250	143	171	90		
+ 120	100	112	100	77	275	209	215	178	274	197	186	113		
+ 180	110	115	103	83	276	220	220	182	280	200	189	119		
+ 120	96	93	71	50	252	194	185	121	259	160	142	65		
+ 60	47	24	84	71	179	107	53	94	171	57	43	1		
- 0	45	-52	-54	-29	195	51	-29	-82	208	77	-33	-51		
- 60	-31	-81	-85	-53	277	88	-69	-130	257	103	-50	-87		
- 120	-24	-87	-95	-68	275	125	-79	-150	280	169	-38	-108		
- 180	00	-86	-97	-73	256	136	-77	-154	282	193	-16	-113		
- 120	-04	-66	-65	-41	253	118	-64	-95	251	155	-01	-63		
- 60	25	-15	-86	-13	180	55	-16	11	176	68	35	-13		
- 0	38	54	51	34	194	59	110	107	209	35	110	49		
	W=145. (Fig. 2.)	W=400. (Fig. 3.)	W=910. (Fig. 4.)	W=1910. (Fig. 5.)	W=82. (Fig. 6.)	W=209. (Fig. 7.)	W=527. (Fig. 8.)		W=273. (Fig. 9.)	W=527. (Fig. 10.)	W=910. (Fig. 11.)	W=1910. (Fig. 12.)		
0	34	57	41	23	107	81	126		92	73	79	26		
+ 60	78	97	75	43	191	168	181		173	159	131	55		
+ 120	99	110	92	59	240	203	200		210	190	150	78		
+ 180	105	114	97	65	251	209	204		221	193	152	86		
+ 120	91	85	61	38	224	176	159		184	149	69	49		
+ 60	39	16	4	76	139	77	44		85	51	15	68		
- 0	21	-60	-46	-16	110	11	-78		87	-19	-85	27		
- 60	-37	-90	-73	-35	180	23	-140		186	-28	-134	-53		
- 120	-37	-98	-84	-49	222	40	-163		236	61	-150	71		
- 180	-36	-99	-86	-56	235	48	-167		257	96	-151	-77		
- 120	-29	-62	-51	-30	210	42	-125		216	83	-98	44		
- 60	-70	30	-37	-11	128	26	-17		112	47	-72	42		
- 0	38	61	42	24	108	81	-122		95	64	81	28		

$\delta = 15.78.$					$\delta = 23.5.$				$\delta = 33.54.$			
$r.$	$\zeta.$ W=527.	$\zeta.$ W=1040.	$\zeta.$ W=2010. (Fig. 30.)	$\zeta.$ W=2800. (Fig. 31.)	$\zeta.$ W=2.5. (Fig. 32.)	$\zeta.$ W=1070. (Fig. 33.)	$\zeta.$ W=2140. (Fig. 34.)	$\zeta.$ W=2.5. (Fig. 35.)	$\zeta.$ W=1070. (Fig. 37.)	$\zeta.$ W=2140. (Fig. 38.)	$\zeta.$ W=2820. (Fig. 39.)	
0	200	35	3.5	- 8.2	268	97	0.4	279.8	88	19	7.9	
+ 60	244	153	1.4	- 22	274	160	12	281.1	147	60	5.1	
+ 120	259	200	16	- 24	276	186	74	280.2	176	95	32	
+ 180	262	212	30	- 22	275	195	102	279.8	188	107	46	
+ 120	197	141	30	- 4	263	131	66	276.7	148	70	36	
+ 60	115	51	29	14	255	53	23	277.7	84	30	23	
0	199	31	46	36	269	99	- 0.4	280.5	90	18	17	
- 60	244	132	189	60	273	161	12	281.4	149	67	28	
- 120	261	204	127	...	275	187	73	281.1	177	107	66	
- 180	266	216	141	83	273	196	106	279.8	188	120	82	
- 120	204	148	99	52	261	131	67	277.0	148	79	57	
- 60	111	55	40	18	252	54	24	276.7	104	34	21	
0	203	32	4.9	- 7.7	268	97	- 0.5	279.8	88	16	7.6	
	W=782. (Fig. 28.)	W=1420. (Fig. 29.)	W=2260.		W=527.	W=1630. (Fig. 33.)		W=527. (Fig. 36.)	W=1630. (Fig. 37.)	W=2690. (Fig. 38.)	W=3210. (Fig. 40.)	
0	123	11	- 3.5		174	18		194	47	5.6	1.1	
+ 60	224	56	- 18		209	102		228	103	18	- 5.3	
+ 120	249	152	- 17		229	141		242	139	40	- 0.4	
+ 180	256	172	- 7.9		236	150		247	152	49	5.3	
+ 120	187	107	0.7		194	86		218	106	34	8.8	
+ 60	81	38	11		124	26		171	49	19	12	
0	131	14	25		175	18		188	48	16	19	
- 60	225	63	52		210	99		225	104	42	34	
- 120	253	164	102		230	143		241	142	73	49	
- 180	260	184	122		236	153		246	154	83	57	
- 120	194	121	79		177	90		218	109	53	37	
- 60	97	44	31		127	29		169	51	23	16	
0	128	13	- 3.3		174	18		193	47	3.9	0.8	

Note by J. T. BOTTOMLEY and A. TANAKADATÉ.

[Plate VI.]

The results described in the foregoing paper are of so remarkable and unexpected a character, that it seemed both desirable and interesting that some of the experiments should be repeated under fresh conditions and by new hands. Accordingly the authors of the present note, though they desire to avoid interfering in any way with an investigation so admirably commenced by Mr. Nagaoka, have made a few observations of which a brief account may here be given.

To each end of a nickel wire a cross-bar of very thick copper strip was attached. A hole was drilled in the middle of the cross-bar and the end of the nickel wire passed into it and silver-soldered. One of these bars was attached by brass screws to the under side of a wooden beam; and to the other bar a scale-pan for carrying a stretching load, and a very simple apparatus for applying pure torsion were connected. The accompanying sketch (Pl. VI.) will explain the arrangement. The torsion-apparatus consists of a brass tube fitting, not too tightly, inside a second outer tube, which is fixed to the table and acts as a guide. The inner tube carries a fork which is connected by two easily removable pins with the copper cross-piece. An enlargement of the torsion-apparatus, T, is shown in the figure. The cord which carries the scale-pan and weights is attached to the cross-bar and passes down along the axis of the inner tube. The whole of the torsion arrangement can be disconnected from the wire in two or three seconds, by removing the pins and the wire left hanging with or without the scale-pan and weights. The wire itself also can with the greatest ease be raised or lowered, or can be completely removed in order to find the zero of the magnetometer. It is only necessary for this purpose to pull out two pins of very thick copper rod which serve to support the wooden beam, A B.

The magnetometer M is of the kind now well known:—A light galvanometer-mirror with magnets on the back, hung by a long single silk fibre. It was placed, to the east of the wire, on the table; and a lamp and scale were employed for reading the deflexions. The magnetometer was placed opposite to the lower end of the coil; and the magnetometer-needles were ascertained to be magnetized in such a way that *true southern* magnetism (red) developed in the lower end of the wire, placed as described, caused increased readings of the magnetometer-scale. The northern magnetism caused dimi-

nished readings. The scale used is graduated from 0 at the left hand to 1000 at the other extremity.

The foregoing brief description of the apparatus used will be considered sufficient in a note like the present.

The nickel wire used was 1.58 millim. in diameter. It is of excellent quality, though not guaranteed perfectly pure. The first wire used was 63 centim. long ; a second wire, which was used inside a solenoid, was 5 or 6 centim. longer. After being hung up, the wire was several times heated to redness at every part by passing a Bunsen-flame up and down it. The scale-pan and a small weight were kept hanging on the wire during this heating, which was with care carried out to the very extremities of the wire.

The wire was then very carefully lifted down, shocks being avoided ; the zero of the magnetometer was ascertained ; the wire was replaced, and the scale-pan first and then the stretching-load were cautiously applied. The heating process was repeated when, after any usage to which the wire had been subjected, it was judged that irregularity of magnetization might have been introduced.

After a few preliminary trials had been made with small loads, all of which seemed to verify conclusions which would be represented by Mr. Nagaoka's curves (figs. 1 and 2), the wire was reheated and a load of 12.49 kilogs., or 640 kilogs. weight per square centimetre, was applied. This it was considered ought to give results such as would be represented by fig. 3 of the original paper. The following table shows the results of this experiment, and the readings under twist fully confirm Mr. Nagaoka's corresponding experiment.

TABLE I.

Magnetic field	0.46 C.G.S.*
Magnetometer zero	500
Wire in position with very small stretching- weight	} 635

Load gradually applied.

Load, in kilogs.	Magnetometer- reading.
3.63	620
5.41	603
6.84	588
9.77	570
12.49	560

* Calculated from information kindly supplied to us by Prof. Rücker, F.R.S.

Table I. (*continued*).

Twist applied.	
Twist.	Magnetometer- reading.
0	560
+ 45	635
+ 90	740
+135	744
+180	740
+135	657
+ 90	493
+ 45	370
0	310

The wire, on being freed from the torsion-apparatus, was found to have taken a permanent set of about 35° ; being left free to take this position, the magnetometer-reading was 500. Once more brought to zero of the torsion-scale, the magnetometer-reading was 315. It is to be noted that a similar remark as to the permanent set applies in all similar cases during the experiments detailed below. The wire we used would not bear 180° twist in either direction without permanent set.

To alter the magnetism of the wire without applying a magnetizing-coil, a small but powerful steel magnet was applied to the wire, as in the old method of magnetization by "simple touch." The *true* south pole of the magnet was placed against the wire, and the magnet was passed from the highest point downward several times; thus reversing the effect due to terrestrial magnetism.

Magnetometer-reading 215

The magnet was now passed from the lowest end to the top of the wire, the southern pole being still the one in contact with the wire. The effect was to increase very much the effect due to terrestrial magnetism.

Magnetometer-reading 805

Twist was now applied to act upon the residual magnetism which had been acquired during the operations just described. The following is the result ;—

Twist.	Magnetometer.
0	805
+ 90	830
+ 180	823
+ 90	512
0	298
- 45	240
- 90	225
- 180	220
- 90	480
0	730
Wire free from torsion-apparatus	540
Wire removed, magnetometer-zero . .	498

The series thus described having been completed, the wire was replaced with the weight on as before, but the torsion-apparatus was not connected to it. The wire had then a negative permanent set of about 35° . Left free it influenced the magnetometer but little, the reading being 500. Twisting a little (well within elastic limits) from this free position in the positive direction produced increased southern magnetism in the lower end of the wire; twisting to the negative side produced the opposite magnetic effect.

Lastly, so far as this wire was concerned, it was twisted first positively through 180° , then negatively through 180° , observations being taken. The twisting in the negative direction was then carried through another 180° , and finally the negative twist was taken out. The result of this operation was to annul entirely the effect of the first positive twist (say right-handed, see p. 120) and as it were to convert the former positiveness of right-handed twist into negativeness, and *vice versa*.

Cycle of Twisting in the Zero Magnetic Field.

A fresh wire was now suspended, and was subjected to the action of a strong uniform magnetic field obtained by means of a magnetizing-coil with secondary coils. The strength of the field was 182 C.G.S. The stretching-load was small; being only 1.78 kilogs., or 91 kilogs. per square centimetre, or the smallest necessary for keeping the wire stretched in position. After the wire had been thus magnetized the field was reduced to zero (the earth's vertical force being neutralized by means of the solenoid). The change of residual magnetic moment was then observed. This is shown in the following series of numbers :—

Magnetometer-zero	505
Twist.		Magnetometer.
+ 0	778
+ 45	845
+ 90	859
+ 135	855
+ 180	852
+ 135	819
+ 90	665
+ 45	555
0	528
— 45	512
— 90	503
— 135	495
— 180	491
— 135	497
— 90	502
— 105 (free)	498
— 45	529
0	570

The behaviour of the wire was far from being cyclic. The wire was therefore subjected to six complete cycles of twisting; thus, 0° , $+180^\circ$, 0° , -180° , 0° . After this series of operations the magnetometer-readings came to a very nearly cyclic condition, and were as follows :—

Twist	0° .	$+180^\circ$.	0° .	-180° .	0° .
Readings	. .	532	585	499	463	537

Cycle of Twisting in Strong Magnetic Field. Field 101·5, load 1·78 kilog.

Under the action of the coil producing this field the magnetometer-reading was quite out of range of the scale. The spot of light was therefore brought on the scale by means of a fixed magnet placed at right angles to the plane of the magnetometer-needles, so as not to alter the sensibility of the magnetometer. A series of readings under twist were then taken, as follows :—

Twist.	Magnetometer.
0	518
45	425
90	380
135	355
180	323
135	395
90	465
45	467
0	475
— 45	450
— 90	370
— 135	320
— 180	360
— 90	410
— 45	490
0	410

This series shows that, with twisting in either direction in this very strong field and with small load, the magnetization of the wire diminishes instead of increasing. The curve, if traced, would resemble those given for iron by Sir William Thomson (Phil. Trans. 1879, pp. 70–72). A comparison of the various curves of Mr. Nagaoka among themselves had led us to suppose that this might be found to be the case.

Lastly, following up the alteration in form shown in the iron curves by Sir W. Thomson (*loc. cit.*) to be due to increase of stretching-load, a few experiments were made on a piece of the iron wire used by him with the view of finding whether in a very weak field, or almost no field, and with a very heavy pulling-load, the iron wire could be made to behave like nickel, and to show magnetism increased by twist instead of diminished. So far as we have gone, our experiments did not show any likelihood of our being able to produce this condition in the iron. The results obtained corresponded very much in character with those observed by Sir W. Thomson. We do not, however, consider that our experiments, so far as they have gone, are sufficient to settle this important question.

Glasgow, Dec. 1888.

XVI. *On the Use of Steam in Spectrum Analysis.*

By JOHN TROWBRIDGE and W. C. SABINE*.

AMONG the difficulties with which the investigator in the subject of spectrum analysis must contend is that of obtaining a source of light which is free from constituents other than those which are under examination, and is at the same time sufficiently powerful to enable one to photograph the spectra of the latter. The voltaic arc gives a sufficiently strong light to enable one to photograph throughout the visible spectrum ; but the carbons between which the arc is formed are full of impurities, and it is difficult to interpret the spectra obtained by this means ; moreover it is not easy to employ the arc-light for researches in the ultra-violet portion of the spectrum. On the other hand, the spark from a Ruhmkorff coil taken between terminals of metals the spectra of which one wishes to examine, gives us, in general, spectra which are comparatively free from impurities ; but its light is very feeble compared with that of the electric arc ; and even when the spark is obtained by means of a powerful coil excited by an alternating dynamo machine, an hour is necessary to obtain, with a concave grating of 21 feet radius of curvature, a photograph on the most sensitive plate we can obtain of the ultra-violet spectrum of copper at wave-length 2100.

It becomes an important question, therefore, to ascertain whether the time of exposure of the sensitive plate can be shortened by any process ; for the outlay in obtaining one photograph in the ultra-violet by the means hitherto at our command is very large, involving the running of an engine of at least two-horse power for an hour. In our experiments with a jet of steam, we find that the time of exposure of the sensitive plate can be shortened to at least one third.

We were led to employ steam for the purpose of obtaining the spectra of oxygen and hydrogen with a more powerful electrical excitation than is possible in Geissler tubes. During the winter of 1886 one of us, in connexion with Mr. C. C. Hutchins, of Bowdoin College, while engaged upon the investigation of oxygen-spectra, tried to obtain a powerful electric spark in an atmosphere of steam ; but the experiments were unsatisfactory. The difficulties were chiefly in the way of defective insulation of the terminals of the Ruhmkorff coil. Our experiments showed that no containing-vessel could be employed ; for the sides of the vessel conducted the electrical

* Communicated by Prof. John Trowbridge.

charge from one terminal to the other. No spark could be obtained and the experiments were abandoned. During the present winter the experiments were renewed. The containing-vessel was abandoned, and the jet of steam was allowed to impinge directly upon the spark. No effect could be perceived when there were no condensers in the secondary circuit, and with the introduction of small condensers the effect was not marked. When the number of Leyden jars (8 in. \times 6 in.) was increased to four, the effect of the jet of steam upon the electric spark was surprising. Its light immediately became comparable with that of the electric arc, enabling one to see metallic spectra with the naked eye upon the ground-glass of the photographic camera without the use of an eyepiece. The chamber in which the spark and steam-jet was placed became rosy red from the hydrogen arising from the dissociation of the steam. The hydrogen and oxygen lines in the air-spectrum became very much strengthened; a continuous spectrum showed itself in the neighbourhood of the C line, and also in the yellow. A photograph of the air-lines and metallic lines of the terminals employed could be taken in a third of the time which was necessary when the steam-jet was not employed.

The apparatus consists merely of a tin box which is placed opposite the slit of the spectroscope. Steam enters at one side and is blown across the terminals of the Ruhmkorff coil, which are placed in the box opposite the slit. An outlet on the side opposite to that in which the steam enters allows the steam to escape into the outer air.

The change of colour of the spark is undoubtedly due to hydrogen. The light filling the box is decidedly red, and the hydrogen-line C flashes out with great brilliancy in the midst of a continuous band of red in the spectrum. The metallic lines of the terminals are also greatly strengthened. The light from iron terminals is especially brilliant. Without the steam the spark between iron terminals seems to consist of a single line of discharge. When the steam is turned on, a great bundle of sparks appear in the midst of a flaring light and the noise of the sparks was greatly increased. These effects can undoubtedly be attributed to increased conducting power of the air-space between the terminals of the Ruhmkorff coil.

The appearance of the spectrum led us to examine the question of the character of the spectrum of the aurora borealis and its connexion with that of aqueous vapour. We believe that the theory that the shifting nature of the northern lights may be due to electrical discharges following

strata of air more or less laden with aqueous vapour has been advocated. The appearance of the spectrum of the electric spark in steam certainly leads one at first to favour this hypothesis. We have spoken of the marked brilliancy of the hydrogen-line, and of a continuous red band near this line. The continuous spectrum in the yellow is no less prominent. The observations which have been made on the northern lights do not enable one to make exact comparisons. The lines given by different observers, however, do not appear to coincide with the prominent lines and bands observed in the air-spectrum heightened by steam.

Other observers, among them Professors Liveing and Dewar, have employed steam to obtain steam-lines: but we have been unable to find any reference to the remarkable economy in time and in waste of apparatus which results in the use of a jet of steam in spectrum analysis when the spark method of obtaining the spectra of metals is employed.

Jefferson Physical Laboratory,
Cambridge, U.S., Dec. 22, 1888.

XVII. *Molecular Refraction*.

By WILLIAM SUTHERLAND, M.A., B.Sc.*

THE object of the present paper is to furnish a theoretical establishment of Gladstone's empirical law connecting the index of refraction of a substance with its density and to consider certain consequences thereof, also to show that Gladstone's law expresses more accurately than any other which has yet been proposed the connexion between index and density. To this latter end it will be advisable to give a sketch of the history of the subject, especially as that history is one of the most interesting of those of the minor branches of physics.

The first indication of a connexion between index of refraction and density is due to Newton, who was led to discover it by his investigation of refraction according to the emission theory. In his 'Optics,' Book II. part iii. he enunciates (chapter x.) thus:—"If light be swifter in bodies than *in vacuo*, in the proportion of the sines which measure the refraction of the bodies, the forces of the bodies to reflect and refract light are very nearly proportional to the densities of the same bodies excepting that Unctuous and Sulphureous bodies refract more than others of this same density." Newton's proof of this proposition may be stated thus in the terminology of the

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modern science of energy :—the main mass of a transparent body may be considered to be a field of constant potential, and so also may the external free æther ; but in the passage from the body to the æther there is a layer of space in which the potential varies along the normal to the layer ; the change in the kinetic energy of a corpuscle of light in crossing this layer must be equal to the determinate change of potential energy corresponding to the passage from one region of constant potential to another. But only the normal component of the velocity changes ; hence we have the result that the difference of the squares of the normal velocities of a corpuscle in æther and in the substance is proportional to the definite change of potential ; or the square of the ratio of the normal velocity in the substance to the normal velocity in æther, *minus* unity, is proportional to the same quantity. Now Newton does not expressly show how the change of potential energy on passage from free æther into a substance should be proportional to the density (this was afterwards done by Laplace) ; but he proceeds to tabulate the indices and densities for a number of bodies and to show that the square of the index *minus* unity divided by the density is roughly the same for them all. He comments on the fact that this quantity has the same value for air as for glass, in spite of the great difference in density between the two substances.

Laplace was able, by the treatment of the subject of molecular force, which he develops for his theory of capillarity, to give precision and definiteness to Newton's proof (*Mécanique Céleste*, book x. chap. i.). If, as in the gravitation theory, the force due to a molecule is proportional to its mass, it is obvious that the force exerted by a body on a corpuscle of light near its surface will be proportional to the density of the substance, assuming that molecular force is sensible at insensible distances and insensible at sensible. Laplace is able to show how it is only at the transition layer between æther and substance that a change of potential occurs, and how the change of potential is expressible as a definite integral proportional to the density. He thus gets the formula $(n^2 - 1)/d = c$, where c is a parameter whose value depends only on the chemical constitution of the substance.

Biot and Arago (*Mémoires de l'Acad.* vii. 1806) undertook the experimental verification of this formula in the case of gases, and found it to hold accurately.

With the establishment of the undulatory theory and the abandonment of the emission theory, the theoretical arguments of Newton and Laplace failed to apply any longer to the actual circumstances, but still Newton's discovery of a connexion

between index and density retained its value as an empirical physical law.

In the course of some researches as to how far $(n^2-1)/d$ could be regarded as constant for liquids when d is changed by heat, Gladstone and Dale (Phil. Trans. 1858) discovered that $(n-1)/d$ is more nearly constant than Newton's expression. Berthelot had drawn attention to the desirability of studying for different substances not only $(n^2-1)/d$, but $M(n^2-1)/d$, where M is the molecular weight. Landolt, doing this with Gladstone's expression, discovered that the molecular refraction of a substance, $M(n-1)/d$, is the sum of the definite atomic refractions or refraction-equivalents of the elements composing it. Subsequent researches by Gladstone showed that certain elements have not a single definite refraction-equivalent, but different equivalents according to their different methods of chemical union with other elements. As this discovery promised to be of value to those chemists who are eager to take advantage of all the light that physical investigations can throw on chemical structure, its further prosecution was taken up by Brühl, who established the important result for structural chemistry that whenever two carbon atoms are connected ethylenewise (or, in the "bonds" terminology, when a carbon atom is united to another by a pair of bonds) the refraction of the molecule is increased by a definite amount above the sum of the equivalents of the atoms in the molecule. Kanonnikoff was able, as a first-fruit of optical chemistry, to show how Brühl's discovery throws immediate light on the debated points of the comparative structure of citraconic, itaconic, and mesaconic acids and of the different terpenes.

A large amount of experimental work had thus been undertaken, all on the strength of Gladstone's empirical law, and no physical theory had been advanced to give the law meaning, when Lorenz of Copenhagen, and Lorentz of Amsterdam, brought out each a mathematical theory of molecular refraction; the former applying his treatment of the ordinary undulatory theory, the latter using the electromagnetic hypothesis, but both coming to the same result, but one quite different in form and meaning from that of Gladstone's law. They found that on these assumptions $(n^2-1)/(n^2+2)d$ ought to be constant for a given body. It would be impossible to give in a brief form an analysis of their mathematical work as presented in the German translations of their papers in Wiedemann, volumes ix. and xi.; but a slight sketch of Lorenz's method is necessary to make clear the following discussion of the merits of his formula in comparison with that of Gladstone.

Lorenz assumes that throughout a heterogeneous non-absorbing medium composed of æther and molecules (the æther being quite uniform in properties), the differential equation for the vibratory disturbance of an element during the propagation of a wave of light may be written

$$\nabla^2 \xi = \frac{1}{\omega^2} \frac{d^2 \xi}{dt^2}, \quad \nabla^2 \eta = \frac{1}{\omega^2} \frac{d^2 \eta}{dt^2}, \quad \text{and} \quad \nabla^2 \zeta = \frac{1}{\omega^2} \frac{d^2 \zeta}{dt^2};$$

where ω is a function of x, y, z , the coordinates of the position of the element when undisturbed; ω for a homogeneous medium is the constant velocity of light in it. Considering only plane waves, he assumes that a solution of the above equations exists in the form

$$\xi = (\xi_0 + \xi_2) \cos(kt - lx - my - nz - d),$$

with similar expressions for η and ζ ; where $\xi_0, \eta_0, \zeta_0, k, l, m, n, d$ are all constants, but ξ_2, η_2, ζ_2 are periodic functions of x, y, z ; ξ_0, η_0, ζ_0 being so chosen that the mean values of ξ_2, η_2, ζ_2 throughout a finite volume are zero. Thus ξ_0, η_0, ζ_0 are the components of a certain constant mean amplitude. Now, although a variable amplitude is thus provided for as the disturbance passes from æther into matter, we see that the condition that k, l, m, n , and d are constant commits us to the assumption of an *invariable wave-length*.

Apart from all mathematical symbols, we may state Lorenz's fundamental assumption thus, that in a medium composed of æther and molecules light may be considered as propagated with a certain mean velocity, but with a periodically varying amplitude of vibration. Now there can be no question as to the fact that a wave of light does pass through such a discontinuous mixed medium with a certain mean wave-length and a corresponding mean velocity, no matter how different the actual wave-lengths in pure æther and in pure matter may be; but a serious mathematical difficulty arises when, in deducing an integral relation from the original differential equation, Lorenz assumes this mean wave-length to hold through the substance of a single molecule, throughout which he performs certain integrations involving wave-length, while for all we know the wave-length in matter may be millions of times smaller than in æther. It appears to me that if there is to be integration over a single molecule and its "domain" of æther, then we must employ a solution of the differential equation which provides not only for variable amplitude, but also for variable wave-length, as we pass from æther into matter. The logical consequence of Lorenz's assumption of a mean wave-length is that, like Cauchy, he must replace the mixed

medium by a certain mean medium. The whole form of Lorenz's final result depends on his application of a mean solution to a discontinuous medium to which it does not actually apply as regards the elements of the medium ; and as this is not justifiable *à priori*, it is interesting to examine how its results are experimentally verified, and the history of physics does not contain a more striking example of the experimental verification of a purely theoretically deduced formula than that obtained by Lorenz and Prytz (Wiedemann, xi.). They examined some 15 or 16 compounds both in the liquid and vapour state, and found $(n^2 - 1)/(n^2 + 2)d$ in almost every case to be practically the same in both states, and, considering the great difference in density of the two states, no more searching single test could have been applied by Lorenz to his theory. On the other hand, when Gladstone's formula $(n - 1)/d$ was tested by the beautifully accurate determinations of Lorenz and Prytz, it was found to fail signally in bridging over the great gap in density between liquid and vapour. This fact at once arrested the attention of those engaged in the investigation of molecular refraction, and Landolt, Brühl, and others proceeded to adopt Lorenz's expression because it possessed two great advantages over Gladstone's, it had a theoretical foundation and a better experimental verification.

But meanwhile Quincke (*Sitzungsberichte der kön. preuss. Akad. der Wissen. Berlin*, 1883, and *Phil. Mag.* 5th series, vol. xvii. 1884) tested the three refraction formulæ of Newton, Gladstone, and Lorenz in another manner, by varying the density not by heat, but by hydrostatic pressure. His method consisted in measuring the change of index of a liquid when subjected to a certain pressure. Now each of the three rival formulæ gives a value for the change of density in terms of the change of index, so that from the optical measurements in each case a value of the compressibility of the liquid could be deduced. Quincke's test consisted in comparing these three calculated values of the compressibility with his own actual measurement. He applied the method to 10 liquids of diverse properties. His results showed that Lorenz's theory is defective. In every case Lorenz's formula gave too small a value for the compressibility. Newton's is too large, while in 6 cases Gladstone's gave too small a value, and in 4 too large. The mean percentage errors, according to the three formulæ, I have found to be -14 for Lorenz's, +17 for Newton's, and -1.6 for Gladstone's.

This ought to furnish logicians with an instructive example in the theory of evidence, that Lorenz's formula stood the test of comparison between liquid and vapour states, but failed

decidedly under the difference of density produced by a few atmospheres' pressure. It should be mentioned that in 1874 Mascart (*Comp. Rend.* lxxviii.) showed, by experiments on the change of index of water under pressure, that Gladstone's formula gives very nearly the correct compressibility, while Newton's fails.

Ketteler, developing the theory worked out by Sellmeier, Meyer, Helmholtz, and himself to account for dispersion, obtains a different form of expression connecting index and density, which, in a recent paper (Wiedemann's *Ann.* 1888), is presented in the form

$$(n^2 - 1)(v - \beta) = c(1 + ae^{-kt}),$$

where v is the molecular domain (usually called molecular volume) and β is the true volume of the molecule, c , a , and k being constants and t the temperature. The hypothesis on which this is based is that discontinuous æther and the molecules may be imagined to be replaced by two homogeneous continuous media both filling the same space, but with an action between the two media partly of a frictional nature. As the above expression involves four arbitrary constants, it necessarily permits of great accuracy in the representation of experimental results. Ketteler as yet has applied it to only two substances, water and alcohol, which are hardly suitable for testing any theory relating to molecular structure, as there is an abundance of experimental evidence to show that their molecular condition is exceptional. With experiments made over a wide range of temperature he has determined the values of the constants, so that we can test his formula as to its power to give the correct compressibility.

Proceeding as Quincke does with the other three formulæ, we find that Ketteler's formula would give for the compressibility the value according to Newton's multiplied by $(v_1 - \beta)/v_1$. In this way we find the following values of the compressibility, multiplied by 10^6 , as compared with the experimental and Gladstone values :—

	Experimental.	Ketteler.	Gladstone.
Water . .	46·14	42·1	46·04
Alcohol . .	101·41	97·2	100·2

So far as these two cases go, Ketteler's four-constant formula gives inferior results to Gladstone's one-constant expression.

After this sketch of the history of the subject the following summary of the conclusions of various experimenters as to

the merits of Gladstone's and Lorenz's refraction-formulæ will be appropriate :—

1. In all cases where the same specimen of a substance has been examined with sufficient accuracy in the liquid and vapour states, the Lorenz formula is markedly superior to Gladstone's.

2. Quinke has shown that, within the limits of experimental error, Gladstone's formula is verified by his experiments with compression, while Lorenz's fails.

3. Landolt finds that in most cases both formulæ hold with considerable accuracy when applied to the change from solid state to liquid, but that Gladstone's has the advantage.

4. Landolt finds that Gladstone's formula leads to more accurate results than Lorenz's when applied to the deduction of the index of a mixture from those of its constituents. Thus while the calculated values of $(n-1)/d$ differ from the experimentally found values by .05 per cent. on the average, and .16 per cent. in the worst case, $(n^2-1)/(n^2+2)d$ departs on the average from experiment by .16 per cent., and .6 per cent. in the worst case. Gladstone's formula is thus decidedly the better of the two for chemical optical analysis. If it is used to determine the amount of a particular substance dissolved in a certain amount of solvent from the determined index and density of the solution, it gives much more reliable results than Lorenz's.

5. Landolt showed that the general results as to refraction-equivalents of the elements C, O, H, and N, and the halogens obtained by the study of the Gladstone expression, still held for the Lorenz formula ; but Brühl was able to show that, from the chemical point of view, Lorenz's formula is preferable, as it allows of the calculation of the refraction-equivalent of a molecule from those of its atoms with a smaller percentage error.

It is thus seen that neither formula is a complete expression for all the physical facts, but that on the whole the Gladstone formula is the nearer of the two to the truth. It will now be shown that Gladstone's formula can be obtained as a first theoretical approximation, and that a second approximation gives an extension and improvement of Gladstone's formula, rendering it capable of expressing the complete relation of index to density.

Consider the path of a ray in a medium composed of evenly distributed atoms between which lies æther with all the properties of free æther unaltered. The ray is supposed capable of passing through the substance of an atom. This is a natural supposition, seeing that the vibrations of matter are

communicated to æther and the vibrations of æther are taken up by matter. But the ray travels more slowly in matter than in æther, so that, for every portion of matter passed through in a certain path, the ray loses time as compared with its time along a similar path in pure æther. Now whatever the shape of the atoms may be, if we imagine them to be very small and very numerous, there is a certain mean distance in the atom which we can in every case imagine the ray to traverse, and on account of the numerousness of the atoms this mean distance is the same for all experimentally possible distributions of the molecules. It is possible that the mean distance may alter with temperature, but, within the limits of refraction-experiments as yet carried out, we may assume the mean distance to be independent of temperature. Under these circumstances we imagine that every encounter of a ray with an atom means the traversing of this fixed mean distance in matter instead of in æther, and a corresponding loss of time. The total loss of time along a path will be proportional to the mean number of encounters, which is directly proportional to the length of path travelled, to the mean sectional area of an atom, and to the number of atoms in unit volume; thus calling s the length of the path considered, l the mean distance through an atom, a its mean sectional area, m its mass, and d the density of the substance, so that the number of atoms in unit volume is proportional to d/m , the number of encounters varies as sad/m . If v be the velocity of light in free æther and V in the matter of an atom, then the loss of time in an atom is $l/V - l/v$. Hence we can write the total loss of time as $\frac{kslad}{m} \left(\frac{1}{V} - \frac{1}{v} \right)$.

But if v' is the mean velocity of light in the atom-strewn medium, the loss of time is equal to $s/v' - s/v$.

$$\therefore \frac{s}{v'} - \frac{s}{v} = \frac{kslad}{m} \left(\frac{1}{V} - \frac{1}{v} \right),$$

$$\therefore \left(\frac{v}{v'} - 1 \right) \frac{m}{d} = kla \left(\frac{v}{V} - 1 \right);$$

$$i. e. \quad (n-1) \frac{m}{d} = kla(N-1) = \text{constant};$$

where n is the index of the medium and N is the index of refraction of the matter of the atom. This is Gladstone's law.

Now la is the volume of the atom, and regarding m/d as the actual measure of the domain of the atom (usually called the atomic volume), and calling these volumes u and U , we

can write the above thus :—

$$(n-1)u = kU(N-1).$$

But if we suppose u to become identical with U , n must become identical with N , and we see that the value of k must be 1, and

$$(n-1)u = (N-1)U, \quad . \quad . \quad . \quad . \quad (1)$$

an equation which could be written down direct from the consideration that the effect of the scattered molecules in retarding the light is the same as if the molecules were gathered into a continuous homogeneous mass. The corresponding equation in Lorenz's theory is

$$\frac{n^2-1}{n^2+2}u = \frac{N^2-1}{N^2+2}U;$$

and it is important to notice the great difference in the physical bearings of the two expressions. If N is large, that is if the velocity of light in a molecule is small compared to that in æther, then $(N^2-1)(N^2+2)$ approaches the limit 1, and

$$\frac{U}{u} = \frac{n^2-1}{n^2+2},$$

and this last fraction, for most liquids, has a value about one fourth, so that if N could be shown to be large, we should have the actual volume of an atom about one fourth of its domain; while, according to equation (1), if N is large, U/u is a small fraction of $(n-1)$, and would be small for most liquids. The two theories are thus in direct contradiction to one another, and it would appear that the assumption previously pointed out as having been made by Lorenz, is responsible for the discrepancy, seeing that the greater N is, the more unjustifiable is that assumption.

The general method of proceeding in the previous proof, namely, that of considering loss of *time*, has a certain claim to be considered the correct one, if we remember that, according to the wave theory of light, a ray passes from one point to another by the path which makes its time of passing a minimum.

If we suppose several sorts of atoms distributed throughout the same space, whether mechanically mixed or chemically united into molecules, we ought, according to the above theory, to have

$$(n-1)u = \Sigma(N-1)U; \quad . \quad . \quad . \quad . \quad (2)$$

but only so long as we assume that neither N nor U , for any sort of atom, is altered by chemical combination; if N or U

is altered, then in this last equation it must have its value proper to the compound.

Thus we have the theoretical view of the application of Gladstone's empirical law to mixtures and chemical compounds.

But so far the reasoning by which equations (1) and (2) have been established is only a first approximation to an accurate argument, as we have left out of count the effect of the atoms in producing delay, not only when the wave is passing through their actual substance, but in the intermediate æther by breaking up the wave-front; in other words no ray can get through in a straight line, it is bent now this way, now that, by the successive atoms. The mean ray, therefore, has its time of traversing the æther thus increased. A more definite idea of this effect of the atoms can be obtained if we imagine them spread out in successive planes parallel to the plane of an incident wave. At each encounter with a layer of atoms the plane wave-front, after getting just through, is a curved surface that tends to recover its plane form before encountering the next layer; during this process of recovering the mean velocity of the wave-front must be less than the velocity of a plane wave in pure æther. The amount of delay thus produced must be proportional to the length of path and a function of the density, and as it vanishes with the density we may assume that it is expressible by $s(bd + cd^2) +$, where b and c are constants, which may depend on the arrangement of the atoms in a molecule. Adding this to the right-hand side of our original equation, we get

$$\frac{s}{v'} - \frac{s}{v} = \frac{slad}{m} \left(\frac{1}{V} - \frac{1}{v} \right) + (bd + cd^2 +)s,$$

whence we get

$$(n-1) \frac{m}{d} = la(N-1) + m(b + cd +). \quad . \quad . \quad . \quad (3)$$

This would make the refraction-equivalent of the molecule equal to a constant term *plus* a term proportional to the density if the series can be arrested at the second term. The terms mb and mcd must be small compared to $la(N-1)$, as is seen from the nature of the argument by which they were introduced, and it is to be noticed that they may vary with the structure of the molecule.

To test the last formula (3) the only experiments suitable are those of Lorenz (Wiedemann, xi.), who determines the values of the index and density of six liquids at the tempera-

tures of 10° and 20° , and in the gaseous state at 100° with the utmost accuracy. If, then, in the formula

$$\frac{(n-1)}{d} = \left(\frac{la(N-1)}{m} + b \right) + cd$$

we determine c and the constant bracketed term from the *data* for liquid at 10° and 20° , the value of the bracketed term ought to be found to be the same as the value of $\frac{n-1}{d}$ found by Lorenz for the gas, seeing that d is so small that the term cd may be considered to vanish in the case of a gas. When it is remembered how small the difference in density is for 10° difference in temperature with liquids, it will be seen how strict the test is.

In the following table are given the values of $(n-1)/d$ as calculated by Landolt from Lorenz's data (*Ann. der Chem.* 213) for the different liquids at 10° , 20° , and the vapours at 100° . Water and ethyl alcohol are not included on account of the exceptional character of their molecules; the fifth column contains the values for the vapours calculated according to the above equation:—

Values of $(n-1)/d$.

Substance.	Liquid.		Vapour.		c .
	Observed.		Observed.	Calculated.	
	10° .	20° .	100°	100° .	
Ethyl oxide	·4935	·4930	·4599	·461	·044
Ethyl acetate	·4174	·4172	·4024	·407	·017
Ethyl iodide	·2663	·2658	·2356	·223	·022
Chloroform	·3000	·2996	·2694	·265	·023
Carbonic disulphide	·4977	·4970	·4348	·435	·050

The agreement between observed and calculated values in the above table in all cases except that of ethyl iodide is good, especially if the severity of the method of testing is taken into account. As regards the applicability of the formula to gases under varying pressure we have the experiments of Chappuis and Rivière (*Comptes Rendus*, ciii.). They find

that for air the formula $(n-1) = Ap (1 + \cdot 00058 p)$ represents the index at 21° up to a pressure of 14 metres of mercury, p being measured in metres; and from Van der Waal's equation for air they deduce the relation

$$d = A'p(1 + \cdot 00065 p) ;$$

but this is erroneous, as the correct deduction from Van der Waal's equation is

$$d = A'p(1 + \cdot 00077 p).$$

The equation which I have given, however (Phil. Mag. Aug. 1887), for air, and which is founded on Amagat's more extended data, while Van der Waal's was obtained from Regnault's, gives the relation $d = A'p (1 + \cdot 00060 p)$, which within the limits of experimental error makes $\frac{n-1}{d}$ constant, seeing

that, according to the experimenters, there is a possible error of $\cdot 00003$ in the coefficient $\cdot 00058$. This agreement between the optical and the mechanical experiments shows that the formula obtained from Amagat's data for air will be of high accuracy for manometric work.

But we must see how far the modification of Gladstone's formula to make it applicable to vapour affects its applicability to compression experiments. Its effect is to divide the value of the compressibility as used by Quincke by $1 + cd^2/(n-1)$.

Ethyl oxide and carbonic disulphide are the only two bodies in the above table which occur in Quincke's table, and it will be noticed that for these two c is markedly larger than for the others, while there are two out of the four bodies in Quincke's table for which Gladstone's formula gives too large a compressibility. In the case of æther the above modification reduces the calculated compressibility by 7 per cent., and as it is already 1·5 per cent. too high leaves it 5·5 per cent. too small; in the case of CS_2 the reduction is 11 per cent., but as the excess is 3·6 the modified Gladstone formula gives a compressibility 7·4 per cent. too small. But this gives only a rough estimate of the effect of the modification of the formula, seeing that Quincke's specimens of æther and CS_2 must have been far from pure, as his values of the compressibility of these substances are much smaller than those of other experimenters, but he did not desire chemical purity for the end he had in view. Allowing for the larger values of the compressibility of pure æther and CS_2 , we may say that the modified formula gives the compressibilities 5 per cent. smaller than the observed values, and this in cases where the

modifying constant c is specially large. The general conclusion, then, is that the modified Gladstone equation is capable of representing the relation of index of refraction and density under all circumstances within the limits of experimental error. We see how the values of $m(n-1)/d$, tabulated by various experimenters, for various liquids ought to be reduced to the values corresponding to zero density; or in all cases the observations ought to be made on bodies in the state of vapour, if the results are to be as trustworthy as possible to the chemist.

The advantage that the Lorenz formula possesses is that it happens to give the same value for a substance in the liquid and vapour states; and since for vapours

$$(n^2-1)/(n^2+2)d = \frac{2}{3}(n-1)/d,$$

we see that the Lorenz formula gives approximately, when applied to a liquid, two thirds of the *desideratum*, namely the value of $(n-1)/d$ determined on the *vapour* of the liquid. It is on this account I believe that the Lorenz expression has been found by Brühl to prove more accurate than Gladstone's in its chemical applications; in an indirect manner the Lorenz formula applied to liquids gives a measure of the constant term in the modified Gladstone equation.

The most important fact that the study of refraction from a chemical point of view has brought out is that the refraction-equivalent of an atom of an element is not constant but depends on its method of binding with others. Now, according to the theory of our first approximation, the refraction-equivalent of an atom ought not to vary unless an actual change in the physical structure of the atom has been produced; the second approximation introduces a constant b which may depend on structure, but there is evidence which would seem to indicate that b is zero. Thus the following table contains the values of the Lorenz molecular refraction, which I have calculated from the data given by Bartoli and Stracciati (*Ann. de Ch. et de Ph.* 6 sér. vii. 1886) in their valuable table of physical constants for the liquid paraffins; I employ the Lorenz expression for the reason given above. I have added the value for CH_4 calculated from Mascart's data for gases (*Comptes Rendus*, lxxxvi.). The density at 0° , given by Bartoli and Stracciati for pentane (iso), appears to be a misprint, and I have taken $\cdot 6368$ as the density at 16° .

$$\text{Values of } \frac{n^2 - 1}{n^2 + 2} \cdot \frac{m}{d}.$$

Substance ...	CH ₄	C ₅ H ₁₂	C ₆ H ₁₄	C ₇ H ₁₆	C ₈ H ₁₈	C ₉ H ₂₀	C ₁₀ H ₂₂
	6.6	24.8	29.5	34	38.7	43.3	47.9

Differences...	4 × 4.55	4.7	4.5	4.7	4.6	4.6
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Substance ...	C ₁₁ H ₂₄	C ₁₂ H ₂₆	C ₁₃ H ₂₈	C ₁₄ H ₃₀	C ₁₅ H ₃₂	C ₁₆ H ₃₄
	52.4	57.1	61.6	66.1	70.7	75.2

Differences	4.5	4.7	4.5	4.5	4.6	4.5
-------------	-----	-----	-----	-----	-----	-----

This is the most extended series of homologous compounds yet studied, and it is seen that from the first to the sixteenth member the difference in molecular refraction corresponding to a difference of CH₂ is constant; the mean value is 4.57, a number almost identical with 4.56, the value deduced by Landolt as the general mean given by the alcohol, aldehyde, fatty acid, and æther series. This fact and the fact that isomeric bodies of similar structure have the same refraction-equivalent show that structure does not produce a pronounced effect under some circumstances; but when we come to consider the effect produced by the double binding of two carbon atoms or by the change of valency in a polyvalent atom we are face to face with a most important fact which we can explain as due either to the structural term *b* in our formula, or to a distinct change in the physical properties of the atom when it changes its valency (the double-binding of a carbon atom is equivalent to the change of tetrad to triad). But in some cases the difference between the values of the same atom with different valencies, as for example 12 and 20 for iron in the ferrous and ferric states, seems too great to ascribe to structure and makes probable the idea that the actual physical properties of the matter of the atom are different.

For the full discussion of this interesting question we should require a large number of careful measurements on gases and vapours. Those that we possess are too discordant in certain cases amongst themselves to admit of any great accuracy in their discussion. Bleekrode (*Proc. Roy. Soc.* xxxvii., 1884) gives indexes and density for a number of liquefied gases, Mascart for a number of gases and vapours (*Compt. Rend.* lxxviii. and lxxxvi.), and Dulong for a number of gases; but nothing very definite can be deduced from their results except that in the simpler compounds the atoms have sometimes different refraction-equivalents in different compounds.

Returning to the formula of the first approximation,

$$(n-1)m/d=(N-1)U,$$

we get

$$(n-1)/d=(N-1)U/m=(N-1)/D,$$

where D is the density of the matter in the atom; so that we see that the quantity $(n-1)/d$, which is called the specific refractive power of a substance, is equal to the specific refractive power of a single atom of it. Under these circumstances the specific refractive powers of the elements call for study as much as the refraction-equivalents.

The general results arrived at in this paper are, then, that Gladstone's formula is the best yet advanced, that there is a theoretical foundation for it, and that it can be improved to the form $(n-1)/d=A+Bd$, where A and B are constants; that chemists ought to study the refraction of substances in the state of vapour in order to obtain the necessary data for the investigation of refraction-equivalents and the questions of structure connected therewith; as the practical difficulties in the way of measuring the refractive index and density of vapours are great, the values of A and B in the above formula may be obtained by experiments on liquids at temperatures as far apart as possible, and that where a measurement at only one temperature is available the Lorenz formula may be assumed on empirical grounds to give two thirds of the desired value of A , but there is no guarantee that this will always be the case.

XVIII. *The Viscous Effect of Strains Mechanically Applied, as Interpreted by Maxwell's Theory.* By C. BARUS*.

I. *The two Species of Molecular Break-up which promote Viscous Deformation.*

1. **F**OLLOWING the argument which underlies Maxwell's theory of solid viscosity experimentally, I was obliged to take cognisance of two causes which promote viscous deformation in solids†. For any structure will give way under impressed conditions of stress, as a whole or in part, either because the cements are insufficiently strong, or because the bricks are insufficiently strong to withstand it. Similarly the underlying cause of viscous motion is either such structural change in which groups of molecules pass without loss

* Communicated by the Author.

† Phil. Mag. November, 1888.

of identity from an initial to a final configuration ; or it is a break-up superinduced by the disintegration of one or more molecules of each group. Whichever the change may be, it must, from the nature of the problem in general, be distributed uniformly throughout the mass of the solid (§ 9). Even without stress the said change may result from secular subsidence. Moreover, it is conceivable that molecular disintegration may occur in such a way as to elude detection.

Now I have since been able to prove experimentally that in glass-hard steel a change of the viscous quality may be obtained as the result of at least *two* distinct kinds of structural change, probably as the result of the two kinds of break-up here in question. The first section of the present paper purposes to show this by aid of the phenomena of accommodation, or motional annealing, as they may be called more uniformly in keeping with the following work.

2. The term annealing in its most general sense refers to a process by which strained solid structure, whether maintained by mechanical or chemical causes, is changed to isotropic structure. In viscosity, inasmuch as strained structure is ultimately accompanied by molecular instability, annealing is a process by which viscosity is increased ; and from this point of view annealing need have no direct reference to exposure to temperature. Hence I have designated by the term motional annealing all such forced molecular motion to and fro, in virtue of which the molecules of a thoroughly soft solid are brought into new relations to each other, to the effect that viscosity is increased at the expense of the motionally less stable configurations of the soft solid. There may appear to be some incongruity in the term, inasmuch as the solid motionally acted on always experiences strain ; it is not the strain, however, but the *increment of viscosity* of the solid, to which the term refers*.

Again, in order that a solid may be motionally annealed the mechanical treatment (torsion, traction, &c.) must be applied *below* a certain critical limit of intensity. Otherwise this treatment introduces its own specific instability ; and in proportion as stress is indefinitely increased, the viscosity of the solid may now be reduced in any measure. Further justification of the term is to be given in §§ 6, 9. At present it is more expedient to indicate the points of crucial difference between motional annealing and thermal annealing in their effect on steel in the glass-hard state.

3. When glass-hard steel is annealed at 100°, the effect is

* Phil. Mag. [5] xxvi. pp. 199 to 203, 1888.

a decided increase of viscosity amounting to almost one half of the total viscous interval, hard-soft*. This marked increment of viscosity is accompanied by an equally striking electrical effect†. For it has been shown that the specific resistance of hard steel diminishes as much as 15 per cent. as the result of annealing at 100° . Finally, since the electrical effect is a sufficient indication of the changes of volume (decrement) and of carburization, it appears conclusively that the underlying cause of the increase of viscosity here in question is a disintegration of the unstable carbide molecule of steel.

4. Again, an increment of the viscosity of glass-hard steel comparable in magnitude with that of annealing at 100° , may be obtained without heat. It is merely necessary, for instance, to apply to the glass-hard wire large enough rates of twist, a sufficient number of times alternately, in opposite directions. This method of increasing viscosity has no electrical concomitant comparable with the electrical effect of § 3. Hence the underlying cause of the observed increase of viscosity in this case is probably not a disintegration of the carbide molecule of steel; or at least a disintegration quite unlike that of the foregoing instance.

5. In the following tables (I. and II.) I give the data necessary to substantiate § 4. The method‡ of experiment being identical with the one described in verifying § 3, I need only call to mind here that my normal steel wire (maximum viscosity) and the fresh glass-hard wire (minimum viscosity) were countertwisted; that the ends of the vertical system were fixed, and a mirror-index placed near the middle, at the junction of the two wires. Cf. figure 1, § 12. Table I. contains the results for viscosity, both wires being at the same temperature θ . The impressed rate of twist is given in radians under τ , and the permanent torsion observed at the close of each experiment under $2(\phi + \phi')$. Since the lower end of the system was twisted 360° , $\tau + 2(\phi + \phi') = 2\pi/L$, where L is the length of the two wires. Finally $(\phi - \phi')/\tau$ is the viscous motion at the index, per unit of τ , per unit of L , at the time specified. In other words 2ϕ and

* Am. Journ. Sci. [3] xxxiii. p. 26, 1887; Phil. Mag. [5] xxvi. p. 188 *et seq.*, 1888.

† Bull. U.S. Geological Survey, No. 14, p. 49, 1885.

‡ Phil. Mag. [5] xxvi. pp. 189 to 191, 1888. The use of differential methods premises that the viscous deformations of all the wires to be compared are similar time functions. That this is the case must be verified preliminarily by some absolute method.

$2\phi'$ refer to the hard and the soft wires respectively, and denote angular viscous motion of one right section relatively to another, when their distance apart is 1 centim. of length of wire. $(\phi - \phi')/\tau$ need not be independent of τ . (§ 8.)

Table I. indicates that the wires were twisted 8 times alternately in opposite directions. Before and after each series of viscous measurements in Table I., the resistance of the glass-hard wire was measured by a special device utilizing Matthiessen and Hoskin's method. These results are given in Table II., where r_t is the observed resistance in microhms, Δr_t the variation from the final value, $r_t = 39500$.

TABLE I.—Viscous Effect of Motional Annealing. Glass-hard Steel. $\theta = 29$. $L = 30$ centim. + 30 centim. Diameter of wires, $2\rho = .081$ centim.

Twist No.	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	Twist No.	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.
1.	+·0992	h m 2 25 30 32 35 38 46	+·00 ·94 1·99 2·79 4·45	5.	+·1021	h m 3 40 42 44 47 52	+·00 ·68 1·38 2·19
	+·0055				+·0026		
2.	-·0983	2 49 51 53 56 63	-·00 -1·95 -3·97 -5·93	6.	-·1018	3 55 57 59 62 67	-·00 -·74 -1·44 -2·39
	-·0064				-·0029		
3.	+·1021	3 07 09 11 14 21	+·00 1·01 1·99 3·36	7.	+·1018	4 09 11 13 16 20	·00 ·64 1·28 1·89
	+·0026				+·0029		
4.	-·1021	3 24 27 29 32 37	-·00 -·91 -1·85 -3·66	8.	-·1018	4 23 25 27 30 37	-·00 -·64 -1·28 -2·32
	-·0026				-·0029		

TABLE II.—Electrical Effect of Motional Annealing.
Glass-hard Steel, $2\rho = .081$ centim.

Twist No.	τ .	r_t .	Δr_t .	Twist No.	τ .	r_t .	Δr_t .
0.	0	39550	+ 50	5.	—·102	39560	+60
1.	—·099	39380	—120	6.	+·102	39530	+30
2.	+·098	39350	—150	7.	—·102	39500	+0
3.	—·102	39570	+ 70	8.	+·102	39510	+10
4.	+·102	39430	— 70				

6. The results of Table I. are in excellent accord with my earlier data ; and the oscillatory march of the viscous increment can be represented in the same way. If the tangents be constructed at the same time-point, in each of the curves of Table I., there appears to be some similarity between the march of these results ($\Delta(\phi - \phi')/\tau$) and the corresponding march of r_t in Table II. But interpreted by the data of § 2, this similarity is only qualitative in kind. In other words, whereas the increment of viscosity due to successive alternations of twist is decidedly greater than one half of the increment of viscosity due to annealing glass-hard steel at 100° , it appears that the electrical effect in the first instance (motional annealing) is practically negligible in comparison with the electrical effect of thermal annealing. In Table II. the total interval of variation of Δr_t is about one half per cent. of r_t ; on the other hand, the variation of r_t due to annealing at 100° is from 10 per cent. upwards. It follows that in glass-hard steel there are two distinct ways in which viscosity may be appreciably increased, a result corroborating §§ 3, 4. Again, if the possibilities of viscous motion are to be fully given, it is essential to postulate groups of atoms, as well as the somewhat less definite groups of molecules, both varying in degrees of stability from point to point of the solid mass. Hence slight positional change of the elements of the atomic configurations, or of the molecular configurations, due either to stress not exceeding a critical value or to mere secular subsidence, must in general involve an augmentation of the viscous quality.

7. Having arrived at this result I desire to inquire somewhat more in detail into the viscous relations of the motional

effect, §§ 4, 5. This is attempted in Table III., in which torsional stress is applied cyclically. The viscous behaviour is studied at each of these successive stages of increase or decrease, as shown in the first column, τ , of the table. As before, the length of the system of two wires, $L=l+l'=30$ centim. + 30 centim.; diameter, $2\rho=.081$ centim. The final column $\Delta(\phi-\phi')$, being the detorsion from the beginning to the end of the first minute after twisting, may be taken as an index of the rate of deformation. In the first and second cycles twist is imparted positively; in the third cycle negatively. To follow the sequence of observations it is sufficient to consult the time-column.

TABLE III.—Viscous Deformation in case of Cyclic Twisting.

First Cycle.

Twist Increasing.					Twist Decreasing.			
Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	$\Delta(\phi - \phi') \times 10^6$.	Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\Delta(\phi - \phi') \times 10^6$.
.000					.000	h m 12 31	- 10
						32	0	
						33	- 10	
						34	- 20	
						35	- 27	
						1 18	- 107	
					30	- 120		
.009	h m 10 36	3	.009	12 26	- 8
	37	0	0			27	0	
	38	3	1.1			28	- 8	
	39	5	1.5			29	- 15	
	40	6	1.9			30	- 21	
.017	10 41	9	.017	12 21	- 5
	42	0	0			22	0	
	43	9	1.4			23	- 5	
	44	15	2.8			24	- 12	
	45	20	3.2			25	- 17	
.026	10 46	14	.026	12 16	- 7
	47	0	0			17	- 0	
	48	14	1.5			18	- 7	
	49	23	2.5			19	- 12	
	50	27	2.8			20	- 15	

Table III. (continued).—First Cycle (continued).

Twist Increasing.					Twist Decreasing.			
Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	$\Delta(\phi - \phi') \times 10^6$.	Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\Delta(\phi - \phi') \times 10^6$.
·035	h m				·035	h m		
	10 51					12 11	— 7
	52	0				12	0	
	53	31				13	— 7	
	54	51				14	—13	
	55	67				15	—15	
·043	10 56				·043	12 06	— 5
	57	0				07	0	
	58	45				08	— 5	
	59	77				09	— 8	
	60	100				10	—13	
·052	11 01				·052	12 01	— 3
	02	0				02	0	
	03	36				03	— 3	
	04	69				04	— 9	
	05	103				05	—11	
·061	11 06				·061	11 56	— 7
	07	0				57	0	
	08	60				58	— 7	
	09	100				59	— 7	
	10	133				60	— 9	
·070	11 11	97	·070	11 51	— 5
	12	0	·0			52	0	
	13	97	3·8			53	— 5	
	14	153	6·1			54	— 5	
	15	207	8·2			55	— 5	
·078	11 16	100	·078	11 46	— 2
	17	0	·0			47	0	
	18	100	3·5			48	— 2	
	19	167	5·9			49	— 2	
	20	237	8·3			50	— 1	
·087	11 21	117	·087	11 41	+ 3
	22	0	·0			42	0	
	23	117	3·7			43	+ 3	
	24	197	6·3			44	+ 7	
	25	263	8·3			45	+10	
·096	11 26	137	·096	11 36	+ 8
	27	0	·0			37	0	
	28	137	3·9			38	+ 8	
	29	233	6·8			39	+21	
	30	307	8·9			40	+31	
·105	11 31	167				
	32	0	·0					
	33	167	4·4					
	34	280	7·4					
	35	397	9·6					

Table III. (*continued*).*Second Cycle.*

Twist Increasing.					Twist Decreasing.			
Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	$\Delta(\phi - \phi') \times 10^6$.	Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\Delta(\phi - \phi') \times 10^6$.
·0					·0	h m 2 26 27 28 29 30 — 0 — 9 — 17 — 23	— 9
·017	h m 1 31 32 33 34 35 0 5 7 11 ·0 ·7 1·2 1·8	5	·017	2 21 22 23 24 25 — 0 — 12 — 19 — 25	— 12
·035	1 36 37 38 39 40 0 7 13 15 ·0 ·5 1·1 1·2	7	·035	2 16 17 18 19 20 — 0 — 5 — 10 — 15	— 5
·052	1 41 42 43 44 45 0 7 10 17 ·0 ·3 ·5 ·9	7	·052	2 11 12 13 14 15 — 0 — 9 — 11 — 14	— 9
·070	1 46 47 48 49 50 0 7 19 30 ·0 ·3 ·7 1·2	7	·070	2 06 07 08 09 10 — 0 — 7 — 7 — 7	— 7
·087	1 51 52 53 54 55 0 15 30 41 ·0 ·5 ·9 1·3	15	·087	2 01 02 03 04 05 — 0 — 2 — 4	— 2
·105	1 56 57 58 59 60 0 67 120 170 ·0 1·9 3·2 4·5	67				

Table III. (continued).

Third Cycle.—Twist in opposite direction.

Twist Increasing.					Twist Decreasing.			
Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\frac{\phi - \phi'}{\tau} \times 10^6$.	$\Delta(\phi - \phi') \times 10^6$.	Rate of Twist, τ .	Time.	$(\phi - \phi') \times 10^6$.	$\Delta(\phi - \phi') \times 10^6$.
·017	h m				·0	h m		
	3 24	12		4 17	-28
	25	0	·0			18	- 0	
	26	12	1·9			19	-28	
	27	19	3·0			20	-45	
	28	25	3·9			21	-63	
	29	28	4·6					
·035	3 30	13				
	31	0	·0					
	32	13	1·1					
	33	32	2·5					
	34	37	2·9					
·052	3 35	43				
	36	0	·0					
	37	43	2·3					
	38	77	4·1					
	39	103	5·5					
·070	3 40	97	·070	3 50	- 1
	41	0	·0			51	- 0	
	42	97	3·8			52	- 1	
	43	163	6·5			53	- 1	
	44	213	8·5			54	+ 2	
·087	3 45	147	·087	4 00	+ 5
	46	0	·0			01	0	
	47	147	4·7			02	+ 5	
	48	240	7·6			03	+14	
	49	307	9·8			04	+23	
·096	3 55	117	·096	4 10	0
	56	0	·0			11	+ 0	
	57	117	3·4			12	+ 0	
	58	207	6·0			13	+10	
	59	273	7·9			14	+27	
·105	4 05	135				
	06	0	·0					
	07	135	3·6					
	08	240	6·4					
	09	313	8·3					

8. The first result given by these data is obtained by comparing $\phi - \phi'$ and $(\phi - \phi')/\tau$, showing that the latter quantity is not independent of τ . Nor can it be asserted that the change of $(\phi - \phi')/\tau$ with increasing τ is retarded. It follows that comparable values of $(\phi - \phi')/\tau$ are only obtained by keeping τ nearly constant, as has been done in all my experiments.

Again, the large variations of $(\phi - \phi')$ in the direct or stress-increasing phase of the cycles, as compared with the corresponding variations in the retrograde or stress-decreasing phase of the cycles, together with a repetition of the whole phenomenon on a smaller scale for succeeding cycles of the same sign, are features of these experiments. When the sign of the twist is reversed, cycles which exceed the original one in magnitude are obtained. These in their turn dwindle on repetition of like signs of τ . Finally, the influence of residual action in case of retrograde cycles appears more clearly in proportion as stress approaches zero. On the other hand, the circumflex contours of the earlier retrograde curves (τ decreasing from $\cdot 105$) is not a mere error of observation, but results from superposition of direct and residual phenomena. This is specially marked in the third cycle, where stress, after $\tau = \cdot 087$, was applied in a zigzag way, viz. :

$$\tau \times 10^3 = 87, 70, 96, 87, 105, 96.$$

9. The clue for the interpretation of the above complex phenomena as a whole is suggested by the data for $\Delta(\phi - \phi')$. It then appears that the viscous behaviour of the wires is intimately connected with the amount of permanent set imparted during the period of action of stress. From this point of view the phenomena become not only strikingly analogous to thermal annealing in case of temper, but of special importance as regards their bearing on Maxwell's theory. The following description* applies, *mutatis mutandis*, to both classes of phenomena:—

(1) The viscous deformation (annealing effect) of any stress (temperature) acting on glass-hard steel increases gradually at a rate diminishing through infinite time; diminishing very slowly in case of low stress (temperature); diminishing very rapidly at first and then again slowly in case of high stress (temperature); so that the limit of permanent deformation is approached asymptotically.

(2) The ultimate viscous deformation (annealing effect) of any stress τ (temperature t), is independent of preexisting

* Cf. this Magazine [5] xxvi. p. 214, 1888.

effects of the stress τ' (temperature t'), and is not influenced by subsequent application of stress τ' (temperature t'), provided $\tau > \tau'$ ($t > t'$). In case of incomplete deformation (partial annealing) induced by stress τ (temperature t), this law applies more fully as the ultimate effect of τ (t) is more nearly reached. Again, the effect of τ' succeeding τ (t' succeeding t) is more nearly *nil* as the effect of $\tau > \tau'$ ($t > t'$) approaches completeness.

To discern the cause of this detailed analogy it is sufficient to call to mind, (1) that in thermal annealing viscous deformation is produced by thermal diminution of viscosity under conditions of the initially given stress stored up in the tempered solid; in motional annealing by increase of the mechanical stress applied from without under conditions of initially given viscosity. Finally, (2) the configurations, molecular or atomic, which just break up under the action of stress τ or temperature t , respectively, are necessarily limited by a higher order of stability, and are greater in number than those just surviving under less intense conditions of stress or temperature. This is the crucial feature of the analogy. However unlike the instabilities may be in the two cases of motional and of thermal annealing, however unlike the treatment to which they are here respectively subjected, its effect in *modifying the occurrence* of instability is similar, and hence the similarity of viscous results.

So far as we have observed, however, residual phenomena are absent in thermal annealing of glass-hard steel, and this is a point of difference between the break-up of atomic groups and molecular groups. The former are not reconstructed. Again, in motional annealing for increasing rates of twist, thick wires show viscous deformation at earlier dates than thin wires. The break-up commences at the external surface, where stress is most intense, and proceeds thence to the axis, where stress is least. The history of motional annealing is therefore essentially dependent on the dimensions of the deformed wire and varies for different values of radius. I pointed out* that in soft iron the limits of torsional resilience were reached when the obliquity of the external fibre (shear) somewhat exceeds .003 radians. Finally, the ultimate annealing effect (time = ∞) of any temperature t acting on glass-hard steel increases at a retarded rate with temperature, and practically reaches the limit of variation below 350° . In case of motional annealing stress may be applied in any degree from without, and increasing effects obtained limited

* Am. Journal, xxxiv. p. 183, 1887.

only by the given degrees of resilience or of brittleness. Nevertheless, if the wire admits of permanent set, the analogy pursued may even be pushed to this extreme detail. For, just as in the case of thermal annealing above 350° , no further marked effects are produced, because the intensity of stored stress is invariably below the value of viscosity; so in case of motional annealing, when stress surpasses the limits of resilience, further increment of marked consequence is no longer elastically retained.

II. *Tensile, drawn, and other Strains in their Bearing on Maxwell's Theory of Viscosity.*

10. It is known that the effect of drawing metallic wires through a draw-plate is a marked decrease of the viscosity of the originally soft metal. The diminution increases with the intensity of strain imparted. It is not so well known that the viscous effect of a tensile strain applied in any degree to the same originally soft metal is after straining almost *nil* in comparison. Kohlrausch* and his pupils, Streintz† and others, more recently and in extensive researches Mr. Herbert Tomlinson‡, have occupied themselves with these phenomena. The results of these observers are in general accord, and agree well enough with my work that special publication of new data might appear superfluous. Nevertheless, as steel has been but sparingly dealt with, and as results fitting at once into my diagrams are essential to my purpose, I have found it desirable to communicate them. Apart from these considerations the observations which I need must be made with minute reference to Maxwell's theory. In this respect the earlier work is seriously lacking.

The striking difference in the permanent viscous effect produced by the action of the two strains is particularly surprising, because the strains are imparted by mechanical processes not altogether dissimilar. The action of wire-pulling, however, accompanied as it is by surface compression as well as longitudinal extension is conducive to the permanent retention of high-strain intensities, because it imparts to the

* Kohlrausch's original and fundamental researches are given in my earlier papers. Here I need refer only to Schroeder, *Wied. Ann.* xxviii. p. 354, 1886.

† Streintz, *Pogg. Ann.* cliii. pp. 395, 396, 411, 1874.

‡ H. Tomlinson, *Phil. Trans.* 1886, ii. pp. 801 to 837. The variety of strains and metals examined in this paper give it unique value as regards the subject of the present section.

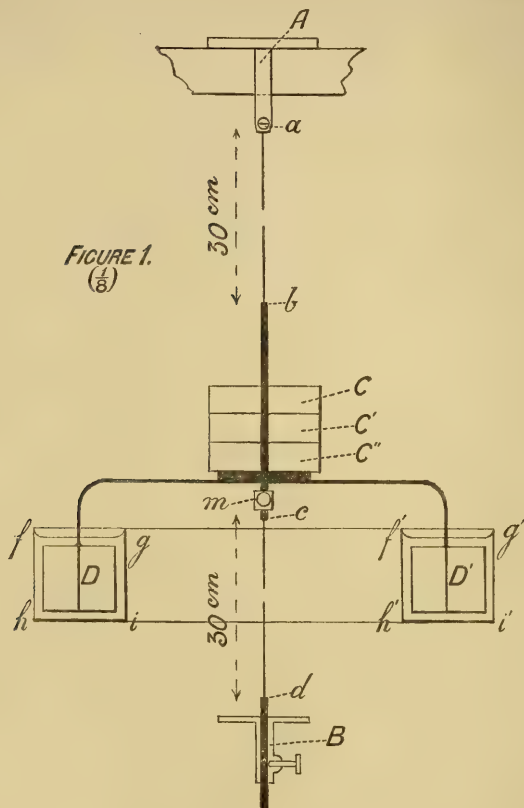
wire an arched structure. In steel, at least, there is a dense shell surrounding an unusually rare core in such a way that the density of the whole mass is materially lessened. Conditions favourable to the retention of high-strain intensities, are also conditions favourable to the occurrence of molecular instability. Hence the marked loss of viscosity of a drawn wire, as compared *cæt. par.* with a soft wire. This premised, it appears that in the case of a wire hardened by simple traction the strain retained after traction is of insufficient intensity to be accompanied by marked molecular instability. More rigorously: if the wire during traction has experienced a strain S , which strain after the tensile stress is withdrawn diminishes to S' ($S > S'$), then the wire need exhibit no change of viscous quality. For in the above cases of thermal annealing and torsional motional annealing (§§ 2, 9), the greater tensile strain S , supposing its action sufficiently prolonged, has wiped out all motional instability for strains $S' < S$; so it follows here that to evoke viscous deformation by aid of tensile strains, the wire must be examined *during traction*, and preferably under conditions of strain near the limit of rupture.

11. The apparatus used in these experiments is shown in fig. 1, in which ab and cd are the two steel wires to be counter-twisted. The system is fastened above and below to two massive torsion-circles, A and B, respectively. The inner ends of the wires are joined by a strong brass rod bc , carrying a symmetrical circular platform near the lower end, on which the lead scale-weights C C' C'' &c. (4 kilogs. to 5 kilogs. each) may be supported. In order to facilitate quick work, the connecting rod bc is provided with a pair of cross-vanes D, D', submerged in the water contained in an annular trough, $fgih$ $f'g'i'h'$. Finally, the mirror m adjustably attached to the rod bc (readings by telescope and scale) indicates the difference of viscous motion of the two wires in consequence of a fixed rate of twist stored between A and B. I may add that the rod bc can easily be chosen light enough, compatibly with strength, so as to introduce no viscous effect of its own.

12. The data to test the above are given in the following tables, of which Nos. IV., V., VI. exhibit the behaviour of some drawn steel wires. In each case the comparison is made with my steel normal No. 15 (the lower wire in fig. 1, An. 450° and twisted to permanent viscous qualities). The drawn wire in Table IV. is in the moderately resilient bright commercial state, very soft to the file. The wire in Table V.

has been drawn down from a larger diameter and the same state of hardness to an extreme of brittle resilience. The wire of Table VI. finally was first softened by heating to redness in air and then drawn down to brittle resilience.

Fig. 1.—Apparatus for comparing Viscous Deformation.



Dimensions are given in the tables, and the arrangement of data is that of the above. θ and θ' are the temperatures of the drawn and normal wires respectively. The former is constant, the latter (θ') at first 30° , and then 100° ; so that the examination is made at two temperatures, in order to compare corresponding viscous effects of the drawn strain and of temper. As above, § 5, $\tau + 2(\phi + \phi') = 2\pi/L$, and $(\phi - \phi')/\tau$ is the viscous deformation indicated by the mirror,

per unit of length of system, per unit of τ . It must be borne in mind that $(\phi - \phi')/\tau$ is a function of τ , for which reason the same rate of twist is applied to the wires throughout; applied, moreover, positively and negatively for each value of θ' . Finally, after testing the wire in the drawn state, it was softened by heating to redness in air and again tested. In this case viscous motion at the mirror nearly ceases, thus affording an excellent check on the validity of the experiments.

TABLE IV.—Viscosity of Hard-drawn Steel Wire compared with Soft. $2\rho = 0.82$ centim.; $L = l + l' = 30$ centim. + 30 centim.

Wire drawn moderately resilient.				Same wire annealed at red heat in air.			
θ . θ' .	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	θ . θ' .	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.
30 30	+·1041 +·0037	h m 1 36		30 30	+·1034 +·0014	h m 3 08	
		38	·00			10	+·00
		39	1·08			11	·10
		44	3·33			13	·07
		48	4·43			16	·10
30 30	—·0989 —·0058	1 51		30 30	—·1019 —·0028	3 22	
		53	—·00			24	+·00
		54	—1·01			25	·00
		56	—2·35			27	·03
		59	—3·58			30	·07
100 30	+·0989 +·0058	2 24		100 30	+·1047 +·0000	4 00	
		26	·00			02	·00
		27	2·83			03	·33
		29	6·53			05	·78
		32	10·23			08	1·31
100 30	—·0931 —·0116	36	13·60			12	1·44
		2 39		100 30	—·1004 —·0043	4 13	
		41	·00			15	—·00
		42	3·03			16	—·33
		44	7·07			18	—·88
		47	10·77			21	1·65

TABLE V.—Viscosity of Hard-drawn Steel compared with Soft.
 $2\rho = \cdot 082$ centim.; $L = l + l' = 30$ centim. + 30 centim.

Wire drawn ($2\rho = \cdot 13$ cm. to $\cdot 08$ cm.) very resilient.				Same wire annealed at red heat in air.			
θ . θ' .	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	θ . θ' .	τ . $2(\phi + \phi')$.	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.
30 30	+·0959	5 52 54 55 57 60 63	 ·00 4·37 10·10 15·68 19·46	30 30	—·1019	1 32 34 35 37 40 44	 —·00 —·13 —·17 —·20
	+·0087	6 30	34·47		—·0028		
30 30	—·0916	6 32 34 35 37 40	 — ·00 — 4·72 —10·50 —16·16	30 30	+·1034	1 45 47 48 50 53	 +·00 ·10 ·10 ·15
	—·0131	44	—20·88		+·0014	57	·21
100 30	+·0916	6 52 53 54 55 56	 ·00 16·80 27·30 34·50	100 30	—·1019	2 00 02 03 05 08	 — ·00 — ·47 — ·91 —1·28
	+·0131	56·7	39·05		—·0028	12	
100 30	—·0873	6 58 59 60 61 62 63	 — ·00 —16·80 —26·30 —33·70 —39·40		Accident.		
	—·0175						

TABLE VI.—Viscosity of Hard-drawn Steel compared with Soft. $2\rho = \cdot 082$;
 $l = l' = 30$ cm.; $\theta = \theta' = 30^\circ$. Wire drawn ($2\rho = \cdot 13$ to $\cdot 08$ cm.) very resilient.

τ . $2(\phi + \phi')$.	Time.	$10^3 \times (\phi - \phi')/\tau$.	τ . $2(\phi + \phi')$.	Time.	$10^3 \times (\phi - \phi')/\tau$.
+·099	3 40 41 42 43 44 45 46 47	 ·00 3·77 6·39 8·30 9·80 11·10 12·20	+·099	5 17 18 19 20 21 22 23 24 25	 ·00 2·83 4·44 5·80 6·80 7·70 8·40 9·10
—·099	5 02 04 05 06 07 08 09 10 12	 — ·00 — 2·70 — 4·65 — 6·06 — 7·35 — 8·40 — 9·30 —10·90	—·099	5 35 36 37 38 40 45 46 47 48 49 50 52 56	 — ·00 —2·35 —3·77 —5·80 —8·90 ·00 2·10 3·36 4·30 5·73 7·70
—·006			+·006		

In Table VII. I give results for the viscous effect of moderate values of tensile strain, on a plan identical with the above. The pull on the strained wire (the normal being No. 15) is indicated in the first column in kilogrammes. The maximum pull which these wires will bear was computed to be between 30 kg. and 50 kg. It was found to be 45 kg. in some cases, experimentally. Inasmuch as I here apply 8.4 kg., the pull applied is 17 per cent. to 25 per cent. of the load producing rupture.

TABLE VII.—Viscous Effect of Moderate Traction. Soft Steel Wire. $2\rho = .082$ centim.; $l = l' = 30$ centim.; $\theta = \theta' = 30^\circ$; $\tau = .100$.

Pull.	Time.	$10^3 \times (\phi - \phi')/\tau$.	Pull.	Time.	$10^3 \times (\phi - \phi')/\tau$.
kilogs.	h m		kilogs.	h m	
0	5 13		4.8	5 59	
	15	.00		6 01	.00
	17	.03		03	.07
	20	.10		06	-.07
	24	.13		10	-.13
	29	.17		23	-.27
	35	.18		31	-.40
2.4	5 36	.00	8.4	6 32	
	37	.07		47	.00
	38	.13		7 09	-.27
	40	.23		23	-.27
	43	.23		21 00	-.07
	48	.27			
	52	.40			
	58				

The results of Tables IV., V., and VI. show the marked influence of the drawn strain in effecting diminution of viscosity clearly. Wires drawn only moderately resilient are of the same low order of viscosity as glass-hard wires. Again, if the wires are drawn very resilient the reduction of the viscosity of the originally soft wire is enormous. To eliminate the effect of different degrees of softness I also drew down a wire after heating in air (Table VI.). The results are in accord with Tables IV. and V.

Some insight into these results is expeditiously obtained by constructing tangents at like time-points of the curves. This is approximately done in the small summary following (Table VIII.), by subtracting the values of $(\phi - \phi')/\tau$ for the beginning and end of the second minute after twisting. Similar values for stretched wire are given in the last two columns, kg. denoting the load.

TABLE VIII.—Viscous results for the Hard and Soft Wires in Tables IV., V., VI., VII.

θ	τ	Drawn.	Soft.	θ	τ	Drawn.	Soft.
		$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$	$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$			$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$	$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$
30	+10	1.08	.10	30	+10	4.37	.00
30	-10	-1.01	-.00	30	-09	4.72	-.10
100	+10	2.83	.33	100	+09	10.50	.47
100	-09	-3.03	-.33	100	-09	9.50	

θ	τ	Drawn.	kg.	τ	Stretched.
		$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$			$\Delta \frac{\phi - \phi'}{\tau} \times 10^3$
30	+10	+2.62	0	.10	+0.03
30	-10	-2.70	2.4	.10	+0.06
30	+10	+1.61	4.8	.10	+0.07
30	-10	-1.42	8.4	.10	
30	+10	+1.26			

From this table the small effect of traction in case of loads below 25 per cent. of the breaking stress is specially manifest; an effect negligible in comparison with the diminution of viscosity due to the drawn strain. For the sake of orientation I may assume that corresponding values of $\Delta(\phi - \phi')/\tau$ for glass-hardness would at 30° be about $1/10^3$ to $2/10^3$; that is, about equal to this quantity in the case of a moderately resilient wire. In general the results for the drawn strain both at 30° and at 100° corroborate and accentuate the results already obtained for temper*, so that the further discussion can be made as in my earlier papers, and may be waived here. I add only that the effect of drawing in case of steel is a decrease of the density of the soft metal; for instance,

Rods	A.	B.	C.
Originally soft . . .	7.76	7.72	7.68
Drawn very resilient .	7.72	7.64	7.64
Softened after drawing	7.80	7.73	

It appears that in this respect also the drawn strain and temper are similar.

* Phil. Mag. [5] xxvi. p. 205 *et seq.*, 1888.

13. Returning to the case of simple traction, the changes of sign of $(\phi - \phi')/\tau$ suggesting increase of viscosity for loads slightly greater than 5 kg., indicate that here, as in the case of motional annealing due to torsion, the initial effects are an increase of viscosity. Nevertheless the obscurely small viscous variations due to traction do not admit of interpretation, unless supplemented by data for very much larger loads. Again, it is desirable and quite feasible, by aid of the apparatus discussed above, to operate cyclically ; in which case the results must be such as to bear on the lag-quality of solids under stress.

In the following Table (IX.) I have therefore inserted some data for larger pulls. Traction increases successively as far as 90 per cent. of the breaking stress. It will be superfluous to give more than a few typical examples ; and the tables can be further abbreviated, because in the stress-decreasing phase of the cycles the viscous effect of loads P below the maximum employed is *nil*, § 10, and therefore sufficiently given by the final load zero. To retain a fixed rate of torsion, τ , it is necessary to twist the wires anew after each of the larger loads ; in other words to bring back the same scale-division into the telescope at the outset of each experiment. Otherwise the torsion seriously decreases in consequence of the rapid viscous deformation of the loaded wire.

In adding the successive loads care was taken to avoid vibration and jarring ; but without special machinery it is impossible to protect the system completely against it. To this cause I attribute certain irregularities of sequence which these and others of my results exhibit. Their general significance is none the less definite. The effect of traction is diminution of viscosity, increasing at an accelerated rate in proportion as stress approaches the limit of rupture. Indeed by sufficiently increasing stress, viscosity may be diminished in any degree whatever. The singularly curious feature of these experiments is this, that with the removal of load the viscous effect of traction almost entirely vanishes. It is in this respect that the present experiments bear directly on the truth of Maxwell's theory ; for it is only during the interval within which conditions favourable to molecular break-up are *forcibly* maintained that the wires exhibit a low order of viscosity of a sufficiently marked degree to compare with the viscous effect of drawn strains and of temper, where instabilities are structurally retained.

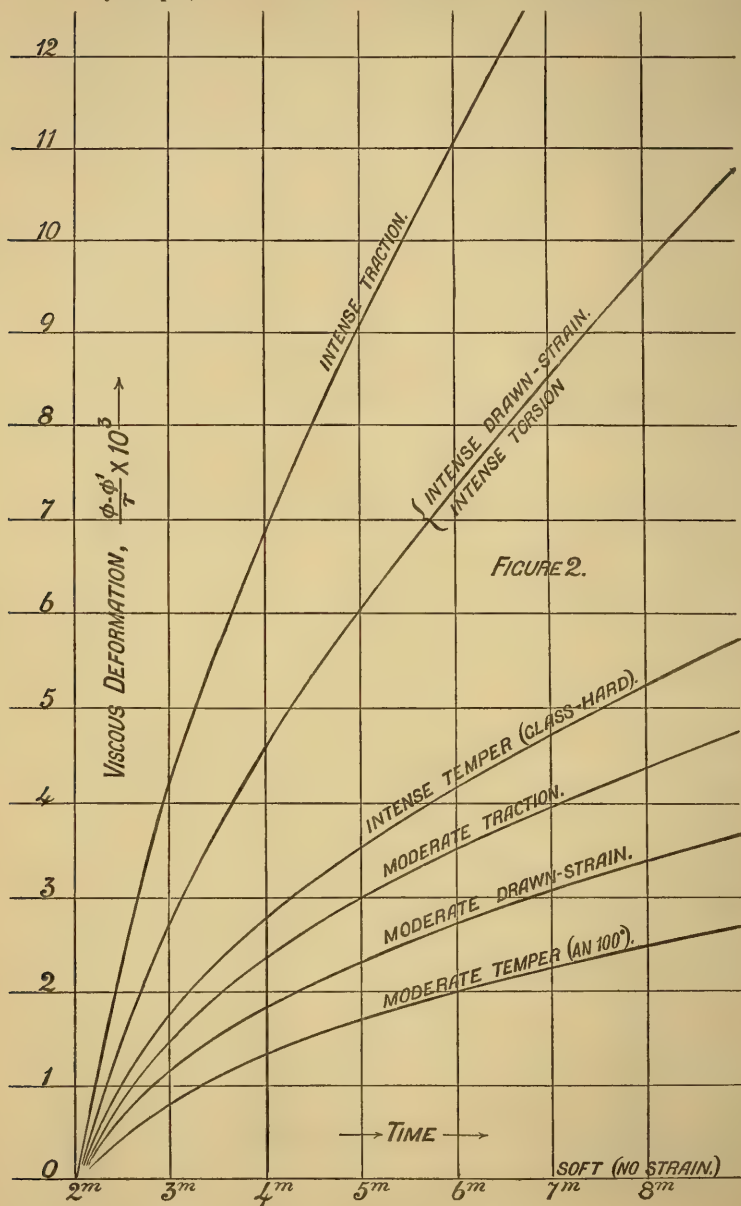
It follows, in general, that slight applications of mechanical treatment (twisting, traction, &c.), inasmuch as they decrease in number the motionally unstable configurations of the soft

TABLE IX.—Viscous Effect of Cyclic Traction. Soft Steel. $2\rho = .081$ centim.; $l = l' = 30$ centim.

P.	τ .	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	P.	τ .	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.	P.	τ .	Time.	$\frac{\phi - \phi'}{\tau} \times 10^3$.
kg. 0	rad. -10	h m 11 31 33 40 51	rad. + .00 +216 +335	kg. 0	rad. +13	h m 10 22 28 31 36 44	rad. .00 .00 .00 .19	kg. 0	rad. +13	h m 12 01 04 09 13	rad. .00 - .13 - .27
9	-10	11 52 53 56 59 64	+ .00 + .23 + .45 + .52	9	+13	10 45 46 49 52 55	.00 .38 .55 .63	20	+13	12 14 15 18 22 24 28	.00 .00 .05 .07 .09
18	-10	11 64 67 71 83	.00 - .12 - .37	18	+13	10 56 59 61 65 68	.00 .05 .08 .12	29	+13	12 30 31 33 36 40 44	.00 .11 .25 .33 .44
27	-10	12 24 27 30 37	- .00 - .37 - .48	27	+13	11 09 10 13	.00 .45	38	+13	12 45 47	.00

Fig. 2.

Time Variation of Viscous Deformation, in case of Soft Steel transformed by temper, drawn-strain, traction, and torsion, respectively.



wire, increase viscosity (motional annealing). If this treatment is intensified beyond a critical stage, in other words if stress is increased sufficiently beyond the limits of resilience and toward the limits of rupture, then viscosity is again seriously decreased. For the action of stress has now become such as to introduce its own specific instability, whereby viscous deformation is again promoted.

14. Having thus substantiated the remarks of § 10, and shown that the accordant results of the earlier observers and my own are such as follow naturally from Maxwell's theory, it is expedient to give a graphic representation of the more important differential data (*i. e.* differences between strained and soft metal for each case of stress) in hand. In figure 2, the viscous deformation $(\phi - \phi')/\tau$, produced by the action of a fixed rate of twist τ , is represented as ordinate, time as abscissa. The material is steel, the originally soft state of which has been transformed by temper (glass-hardness), drawn strain (wire-plate), tensile strain, and torsion respectively. Curves are given for intense values and for moderate values of stress. The nature of the problem precludes greater definiteness as to stress data. In case of traction infinitesimally below the point of rupture, for instance, viscous deformation would occur with such extreme rapidity that its diagrammatic representation would be a vertical line.

Again, in case of traction of sufficiently small value, the curve would be a horizontal line coinciding with "soft," or even a curve below it (motional annealing). In a measure this is true of the other strains; and I have therefore expediently inserted the values for deformations actually found.

The general outcome of the present paper is this, that the effect of strain of whatever kind, applied in sufficient intensity to homogeneous soft steel, is marked diminution of viscosity. Again, inasmuch as the underlying cause of viscous deformation is the occurrence of unstable configurations the number of which is being reduced in the course of viscous motion, Maxwell's theory naturally suggests the applicability of exponential equations for the description of the time relations of such motion. From another point of view it appears that the loss of viscosity experienced by a given metal, under action of a given kind of strain, may not inappropriately be used as a measure of its intensity. Finally, the curious observation, that in all the cases given loss of viscosity has taken place simultaneously with increase of hardness, is one of the suggestive results of the experiments made.

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XIX. *On some Facts connected with the Systems of Scientific Units of Measurement.* By T. H. BLAKESLEY, M.A., M.I.C.E.*

IT seems to me a not unfitting opportunity, as a sequel to Prof. Rücker's suggestion, that we should modify some of our scientific formulæ, to inquire whether, with a change of formulation, we may not advantageously effect a change in some of the units themselves. That the C.G.S. and the practical system of units do not satisfy all the requirements of a perfect system of mechanical units must, I think, have occurred to others as well as to myself; and I mention this, not as an occasion for surprise, for indeed it would have been surprising if any system brought into vogue twenty years ago should still be found even to approach perfection, but rather in order to draw attention to the points in which these systems fail to give satisfaction, with a view to possibly rectifying their shortcomings.

The sense of imperfection may of course arise in the failure to attain to any one of the various ends which it should be the aim of a good system to reach.

I imagine that the following would be generally considered to be among those ends:—

First. All the quantities considered should be so connected by the equations representing the laws of nature, that no coefficients are required in expressing any units in terms of others. [Correlation.]

Secondly. Where quantities are recognized as being essentially of the same nature, they should be measured by the same unit. [Simplicity.]

Thirdly. Such a system should embrace all of the physical ideas which occur. [Comprehensiveness.]

Fourthly. Such a system should, so far as is compatible with other requirements, have its units closely and decimally connected with natural units. [Naturalness.]

Fifthly. The units should, subject to other considerations, agree with established though arbitrary units of measurement, actually or decimally. [Convenience.]

These five heads may be briefly described as:—

Correlation.

Simplicity.

Comprehensiveness.

Naturalness.

Convenience.

I lay no particular stress upon the order in which these ends

* Communicated by the Physical Society: read December 8, 1888.

should be placed, though in my opinion the above order seems the true one, and I think most people would agree in putting Correlation at the top of the list. Its importance is so great that it has in fact been more completely realized than any of the others, most physical ideas having their due formulæ assigned and recognized. It has been the more easy to do this, since the process is absolutely independent of the real units employed. The ideas need only to have their physical nature truly comprehended.

In the second point—Simplicity—there has been much less progress made. In electrical matters, for instance, though in describing a magnitude it may be perfectly well understood what units of length, mass, and time form the fundamental basis of the measurement, it is further necessary to state whether the Electrostatic or the Electromagnetic system is contemplated.

Again, heat is recognized as a form of energy, and yet the two are not referred to the same unit. If they were so, Joule's equivalent would always be unity. The defects in Comprehensiveness have been lately discussed in this Society by Prof. Rücker*. The one with which I have been brought most in contact is the omission of temperature from the systems in use. I think that if generalizations as great as the Electromagnetic Theory of Light are to be made by the help of systems of units, the Thermometer ought at once to be brought in out of the cold region of unsympathetic isolation and independence.

In chemical physics are to be found some notable omissions in atomic heat and electrochemical equivalence. When we examine the units to note how many are founded upon natural magnitudes, we recognize that the second is not a subdecimal part of the solar day, but only the $\frac{1}{86400}$ of that natural unit. To set against this, however, it is a unit which is in common use.

The centimetre is decimally connected with the earth's quadrant, and the unit of density is that of water, the gramme merely being derived from the centimetre and density of water by proper correlation.

In the quadrant-volt second, or practical system, the length unit is brought up to practical coincidence with the earth-quadrant; the second, as before, is the unit of time; and the other quantities are only decimally connected with the C.G.S. I am not aware that any accidental coincidences occur between other units of this system and natural ones, the nearest approach being probably the volt to the E.M.F. of a Daniell cell.

But it is when we compare the units with those of an arbitrary nature perhaps, but in common use, that the failure

* See p. 104 of the present Number.

in actual coincidence or in decimal relation is most striking. The failure to introduce to any great extent the metrical measures of length into England as against the inch is a well-known case. Again, the horse-power and the erg per second are not decimally related.

So inconvenient is this last defect in the department of electromechanics, that Mr. Preece has suggested that engineers should give up the H.P. of James Watt and adopt in its stead 1000 watts. This suggestion has not, so far as I know, met with any support, perhaps because the modifications in the definition of the unit which would be necessary for its clear appreciation from a mechanical engineer's point of view have not been clearly stated. The evil implied in the suggestion is, however, great; and, if possible, something ought to be done to assuage the acute suffering which engineers feel in passing from H.P. to watts.

I think it will be conceded, too, that even the more hardened physicist feels some pangs of passage on the troubled sea of transformation from the *terra firma* of the Electrostatic system to that of the Electromagnetic; and I do not think a rapid answer could be given by many persons to the question, What is the value, in C.G.S. electrostatic units, of a current of (say) 33·3 practical electromagnetic units? Yet in following out the experiments of Dr. Hertz, with which Mr. Tunzelmann has made us familiar, such a calculation must, I think, be made.

I will here give two formulæ, which will enable any magnitude given in one system to be brought into the other.

Let k = the exponent of l in electromagnetic system,

$$\begin{array}{llllll} h = & & & m & & \\ n = & & & l & \text{in electrostatic system,} & \\ q = & & & m & & \end{array}$$

for any kind of magnitude brought into due correlation with others.

Then, to reduce between practical (quadrant-volt-second) units electromagnetic, and C.G.S. units electrostatic :

$$1 \text{ practical unit} = 3^{n-k} 10^{10n-11q-k} \text{ electrostatic C.G.S.}$$

To reduce between C.G.S. electromagnetic and C.G.S. electrostatic :

$$1 \text{ C.G.S. electromagnetic unit} = 3^{n-k} 10^{10(n-k)+11(h-q)} \text{ electrostatic C.G.S. units.}$$

The 3 in these equations is meant as a sufficient approximation to the number 2·998, which is one tenth of “ v ” in quadrants per second.

I have myself not seen such reduction-formulæ elsewhere, and under the present systems I think they may be found of use.

But it would be far better than employing such formulæ to have but one system for all measurements, and do away with this inconvenient bimetallism in the currency of science.

And in this case the way out of the difficulty is easy. We have only to adopt "*v*" as the unit of velocity. With this one restriction any magnitude will be expressed in a system based upon any units by the same number in either system. Such a system would be a veritable coalition, and for distinction might be so called.

A coalition system, then, merely requires that the unit of velocity shall be a certain one, which is also a natural one. It reduces the degrees of freedom in the choice of units by 1, and, without prejudice to Prof. Rücker's method of formulation, will allow us to fix almost any two other units arbitrarily. The two cannot of course be time and length, for velocity simply involves these alone. If the second were retained, the unit of length would be 30 quadrants. If the quadrant were retained, the unit of time would be $\frac{1}{30}$ of a second.

If it be urged that a velocity of 30 quadrants per second is a large one, I would reply that it is only 30 times the present unit of velocity in the practical system; and that experience shows that extremely high or low units do not materially affect calculations. The unit of capacity is a million times the microfarad of our laboratories, and that of velocity about a million times the speed of an express-train. The use of mega- and micro- has now been found by experience to present no difficulty or objection.

Thus this change in the unit of velocity would not introduce inconvenience in itself; but unfortunately, since $v = \frac{l}{t}$, its change would necessitate one indirectly in either length or time, and clocks and meters are so commonly in use that any interference in their present constructions would be felt a serious thing. Of the two changes, that of the time unit seems to me simplest. To take $\frac{1}{30}$ of a thing which itself is $\frac{1}{60}$ of a minute, that again being $\frac{1}{60}$ of an hour, does not appear so very radical an operation.

If we calculate the various units obtained by this change in the time unit and compare them with the quadrant-volt-second units, we shall find (column V.) that mass, length, and quantity of electricity remain unaltered; time and conductivity become $\frac{1}{30}$; velocity, momentum, current, resistance, magnetic field, pole, and magnetic moment, 30 times; force,

acceleration, energy, and E.M.F., 900; and power, 27,000 times of the practical units; while capacity sinks to $\frac{1}{900}$ of a farad.

This would, however, leave the connexion between the unit of power and 1 horse-power bad, for 1 unit would be about 36.2 H.P.

The correlation of temperature with other units would be, I imagine, best effected by the definition that a unit-of-temperature change shall be that through which a unit of energy in the form of heat would raise a unit of matter possessing unit specific heat; in other words, the equation

$$ml^2t^{-2} = Jmc\theta$$

should be satisfied by $J = 1$.

Since heat is recognized as energy, J must be numerical, notwithstanding arguments have been adduced against this view. It is sometimes said that J cannot be numerical because it changes with a change of units. This only arises from the fact that heat in the form $mc\theta$ is not properly correlated, because $(c\theta)$ is not so. Those who use this argument should apply it in some other case, when the fallacy would be apparent; *e. g.*

Power in watts = 746 horse-powers,

Power in ergs per sec. = $10^7 \times 746$ horse-powers.

Here the factor has changed with a change of units, but no one would deny its being numerical, or the relation of one magnitude to another of the same nature.

If therefore J is reduced to unity by the above definition, we have $(c\theta) = l^2t^{-2}$ for the dimensions of temperature \times specific heat; and if c is taken as numerical, these become the dimensions of θ .

As regards the relation between a degree so obtained and a thermometer-degree, we have the following facts to go upon:—

4.2×10^7 ergs raise 1 gram of water 1° C.,

\therefore 1 erg raises 1 gram of water through $\left(\frac{1}{4.2 \times 10^7}\right)^\circ$ C.

$\therefore 1^\circ$ C. = 4.2×10^7 degrees C.G.S. (water).

1° F. = 2.3×10^7 " " "

1° R. = 5.25×10^7 " " "

These degrees therefore do not fit in decimally with the arbitrary scales in use.

But the number 4.2 may be written $\frac{1}{.2381}$. If therefore we use as the standard of specific heat a substance which,

referred to water, will have the specific heat $\cdot 2381$, we shall then have

$$1^{\circ} \text{C.} = 10^7 \text{ C.G.S. (substance in question).}$$

Now Regnault's value for specific heat of air at constant pressure is $\cdot 2375$, Wiedemann's value is $\cdot 2389$. The mean of these is $\cdot 2382$.

If therefore we adopt the specific heat of air at constant pressure as the standard of specific heat, we have

$$1^{\circ} \text{C.} = 10^7 \text{ of } 1 \text{ C.G.S. (air) unit.}$$

The practical unit would be 10^{18} of a C.G.S. unit, and therefore would be 10^{11} of a degree Centigrade.

In relation to the thermometer, air has other well-known advantages over liquids, and is already the standard for specific inductive capacity and magnetic permeability.

This decimal relation of the Centigrade thermometer would not exist if the coalition of the Electromagnetic and Electrostatic systems were carried out on the lines I have indicated, viz. by making $\frac{1}{30}$ of a second the unit of time. The new unit of temperature would then be 900 times that of the electromagnetic quadrant-volt-second air unit.

Hence

1 unit of temperature	=	900×10^{11}	of 1 degree	Centigrade.
" "	=	1620×10^{11}	" "	Fahrenheit.
" "	=	720×10^{11}	" "	Réaumur.

The endeavour to bring the horse-power and the unit of power into decimal relation may be guided by throwing the formulæ into terms of length, time, and power; and I have accordingly given a table of the ideas involved on this basis, in column VII. Any magnitude which appears in these formulæ unencumbered by the symbol of power will of course be unaltered by a mere change in its unit. Hence, if we adopted as the unit of power, $\cdot 746$ of the watt or a decimal multiple of that quantity of power, we could bring the unit of power into decimal relation with the horse-power without altering the units in the practical system of time, length, velocity, acceleration, resistance and conductivity, and capacity. Thus the expensive standards of our laboratories, of resistance and capacity, our resistance-boxes and our microfarads, would remain to us. But our boxes of grammes would fall in value. The unit of temperature, however, would not be altered, so that Centigrade thermometers would remain. Column VIII. expresses the results of such a change.

No doubt if we did not suffer from an hereditary duodecimal taint, we might discard the $\frac{1}{86400}$ of a day, called a second, and adopt the $\frac{1}{100000}$ of a day as our unit of time,

and that interval would be $\cdot 864$ of a second, which number happens to be the square root of $\cdot 746$; and this would give us a coincidence in magnetic field, whose formula is $v^{-\frac{2}{3}} t^{-1} p^{\frac{1}{3}}$. But magnetic field is not a magnitude of which we keep instrumental standards, as we are wont to do of resistance, capacity, mass, time, length, and sometimes of E.M.F.

It will be observed that in the formulæ the symbol of power occurs either simply or raised to the power of $\frac{1}{2}$, and we may bring power into decimal relation with horse-power either by taking $\cdot 746$ of the unit or $7\cdot 46$. The square roots of these relations are $\cdot 86373$ and $2\cdot 73136$. Using the latter number we should obtain no further coincidences among the magnitudes.

In reviewing the results indicated by the above calculations, in dealing with the units of science in such a way as to obtain :—

- (1) The correlation of the thermometer ;
- (2) The unity of the two systems, Electrostatic and Electromagnetic ;
- (3) The bringing of the unit of power into decimal relation with the horse-power ;

I come to the conclusion:—

(1) The thermometer Centigrade may be brought into decimal relation with temperature-units on the C.G.S. system or quadrant-volt-second system by adopting the specific heat of air at constant pressure as unity.

(2) That such decimal relation is compatible with a change in the unit of power to $\frac{1}{1000}$ of 1 H.P., which latter change could take place without changes in the units of time, length, velocity, resistance, and capacity, but would involve a change in the important standards of mass.

(3) That the union of the two electrical systems could be brought about in the simplest way by taking $\frac{1}{30}$ of one second as the unit of time ; such a change retaining the gramme-boxes, and the metre decimally, and the coulomb, but requiring the unit of capacity to be $\frac{1}{900}$ of 1 farad and the unit of resistance to be 30 ohms.

To my mind the last change recommends itself. No real change would be necessary in the face of the chronometer, or in the metre, or in the gramme-boxes. Each microfarad would have to be called $\cdot 0009$, and each second 30, and every 30 ohms 1 unit. A habit has arisen of attaching particular names to units. This, without doing very much good, adds greatly to the stability of the unit, and makes any subsequent change so much the more difficult. The practice has almost always been confined to electricians, somewhat too oblivious of the mechanical basis of a proper system.

I. Electrostatic formula in l, t, m .	II. Idea.	III. Electromag- netic formula in l, t, m .	IV. Practical Units in terms of C.G.S. Electro- magnetic.	V. Coalition Units, Quadrant, $\frac{1}{10^{11}}$ in gramme of Prac- tical Units.	VI. Coalition Units, as in V., in terms of C.G.S. Electro- static Units.	VII. Electromag- netic formula in l, t , and p (power).	VIII. Electromag- netic Units, Quadrant, Second, 746 Watt, in terms of Practical Units.	IX. Electromag- netic Units, Quadrant, Second, 746 Watt, in terms of Prac- tical Units.	X. Coalition system, Quad- rant, $\frac{1}{10^{11}}$, 746 Watt, in terms of Practical Units.
l^{-1}	Time.	t	1	$\frac{1}{10^{11}}$	$\frac{1}{10^{11}}$	t	1	1	$\frac{1}{10^{11}}$
$l^{-1} m$	Mass.	m	10^{-11}	1	1	$t^{-2} p$	746	746	0000276
$l^{-2} m^{\frac{1}{2}}$	Length.	l	10^9	1	1	l	1	1	1
$l^{-1} m^{\frac{1}{2}}$	Velocity.	l^{-1}	10^9	30	30	l^{-1}	1	1	30
$l^{-2} m$	Force.	$t^{-2} m$	10^{-2}	900	900	$t^{-1} p$	746	746	02487
$l^{-2} m$	Acceleration.	t^{-2}	10^9	900	900	t^{-2}	1	1	900
$l^{-1} m$	Momentum.	$t^{-1} m$	10^{-2}	30	30	$t^{-1} p$	746	746	008289
$l^{-2} m$	Energy.	$t^{-2} m$	10^7	900	900	t	746	746	02487
$l^{-3} m^{\frac{1}{2}}$	Power.	$t^{-3} m^{\frac{1}{2}}$	10^7	27000	3	p	746	746	746
$l^{-2} m^{\frac{1}{2}}$	E.M.F.	$t^{-2} m^{\frac{1}{2}}$	10^8	900	9×10^{10}	$t^{-\frac{1}{2}} p^{\frac{1}{2}}$	864	2731	4731
$l^{-1} m^{\frac{1}{2}}$	Current.	$t^{-1} m^{\frac{1}{2}}$	10^{-1}	30	10^{-9}	$t^{-1} p^{\frac{1}{2}}$	864	2731	1577
l^{-1}	Resistance.	t^{-1}	10^9	30	10^{-9}	t^{-1}	1	1	30
l^{-1}	Conductivity.	t	10^{-9}	$\frac{1}{30}$	30×10^9	$t^{-1} p$	1	1	$\frac{1}{30}$
$l^{-2} m^{\frac{1}{2}}$	Quantity.	$m^{\frac{1}{2}}$	10^{-1}	1	3×10^9	$t^{-\frac{1}{2}} p^{\frac{1}{2}}$	864	2731	00526
l^{-1}	Capacity.	t^2	10^{-9}	$\frac{1}{10^9}$	10^9	t^2	1	1	$\frac{1}{10^9}$
$l^{-2} m^{\frac{1}{2}}$	Magnetic field.	$t^{-1} m^{\frac{1}{2}}$	10^9	30	90	$t^{-1} p^{\frac{1}{2}}$	864	2731	1577
$l^{-2} m^{\frac{1}{2}}$	Pole.	$t^{-1} m^{\frac{1}{2}}$	10^{17}	30	10^{-1}	$t^{-1} p^{\frac{1}{2}}$	864	2731	1577
$l^{-2} m^{\frac{1}{2}}$	Magnetic moment.	$t^{-1} m^{\frac{1}{2}}$	10^{18}	900	10^8	$t^{-2} p^{\frac{1}{2}}$	1	1	1577
$l^{-2} m^{\frac{1}{2}}$	Temperature.	t^{-2}							900

Sir William Thomson, in a paper on units of measurement, contemplates a time when many of the units we now employ will become things of the past by the introduction of the universal gravitation method of connecting mass with length and force.

The naming of particular units employed for a time in the process of this development cannot but retard its progress, and the unity of science should take place independently of the subdivisions which may exist to-day.

It is highly probable that chemical science may soon require a modification of our present units, and we must not keep combining weights, atomic heat, and so on, uncorrelated with the rest of the body of science because the development of science has taken the order it has done. It might have been quite otherwise.

Dr. Whewell entertained a speculation as to whether the laws of motion would ever have been discovered by an intellectual race of jelly-fish inhabiting a world in which were no solid bodies.

In like manner we may imagine that had Torpedoes, or other animals armed with a controllable electrical equipment, won the race for supremacy against man, the laws of electricity might have been reached before the mechanical laws. Faraday and Maxwell would have preceded Galileo and Newton, and Gymnotus might have exhibited in submarine air-cases that uncanny biped Homo who could give you a curious and mysterious shock with a stick or a stone.

XX. *On Permanent Magnet Circuits.*

By GEORGE HOOKHAM, M.A.*

THE immediate object of my experiments in permanent magnets was a practical one, viz. to obtain a magnetic field suitable for the electromotor part of my electricity-meter. If by their use a constant field of sufficient intensity could be produced, its advantage in point of economy in use would make it of the greatest value. At the same time I was aware that the scientific world did not consciously believe in the possibility of a really constant permanent magnet, except when the magnetic circuit was complete through a magnetic metal.

But it occurred to me that there is one familiar case in which absolute constancy is admitted, and yet this condition is not fulfilled. I refer to the arrangement in which a soft iron ring is cut at two points diametrically opposite, and the

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ends carefully refitted by filing and scraping. If, now, this ring is once strongly magnetized by an electrical current, we obtain a powerful permanent magnet. The magnetic circuit, however, is anything but complete. There are two breaks in it; very minute, no doubt, if their mere dimensions are taken into account; but recent experiments seem to show that, however carefully we may fit together surfaces of iron, there is still an easily measurable resistance which is greater than would be due to the dimensions of the air-space; that there is in fact, in addition, a surface-resistance. Such a resistance is the equivalent of a small current tending to reverse the magnetism of the iron. Now, what enables soft and pure iron to withstand this demagnetizing agency? Apparently nothing but the coercive force of the iron, small as it is. Once magnetized, the softest iron under any circumstances remains at ordinary temperatures a permanent magnet, however weak; and one can only suppose that it is in virtue of this very small coercive force that the soft iron magnet overcomes even the small resistance of well-fitted joints. If this supposition were well founded, I felt almost certain that when very hard magnets of tungsten steel were substituted for the soft iron, air-space resistances incomparably greater might be overcome; and yet the magnetic intensity in the circuit be very considerable, and to all intents and purposes absolutely constant. It was at any rate quite certain that this state of things would obtain, if the power of overcoming resistances in the circuit were at all proportional to the coercive force of the material of the magnets. My anticipations have been completely realized, and I have been able to construct magnets which are truly permanent.

As to the special arrangement, when the object in view, as in this case, is subject to commercial considerations, cost of production makes the use of straight bar-magnets almost imperative. A number of these are enclosed in a brass tube, to the ends of which are attached soft cast-iron arms, curving round towards the pole-pieces, which closely face each other, so as to form a narrow slit in which the thin disk-armature of the meter revolves. The magnetic resistance of the arms is practically negligible. As to proportions I was, in principle, guided by the analogy of the voltaic circuit. There, if we wish to overcome a considerable external resistance, the plan is to pile up the electromotive force by increasing the number of cells in series, paying little regard to the consequent increase of internal resistance. Here the steel magnet-bars constitute the magnetic cell, whose magnetomotive force—to use what is at any rate a most convenient expression—is

proportional to the length of the bars. Hence, in order to overcome a considerable air-space resistance, the actual dimensions must be such that the ratio of the total cross-section of the steel magnet-bars to their length must be very small compared with the ratio of the cross-section of the air-space to the distance between the pole-pieces. Obviously, too, the best proportions would depend on the nature of the steel magnets—an entirely unknown quantity—so that the first experiment had to be made almost at haphazard. Curiously enough I have never been able to improve on the proportions used in this first experiment. I have had some fairly accurate tests made lately, and find that the best relation between the two ratios, as stated above, is about 1 to 70. Thus, if A equal the area of cross-section of the air-space, L the distance between the pole-pieces, a the cross-section of the steel magnets, l the length of the bars, then $\frac{A}{L}$ should equal about $70 \times \frac{a}{l}$.

For example, if the cross-section of the steel magnets is 3 square inches, and their length 6 inches, then the air-space may be 9 square inches, and the pole-pieces $\frac{1}{4}$ inch apart, or $4\frac{1}{2}$ square inches with $\frac{1}{8}$ inch distance, or $2\frac{1}{4}$ square inches with $\frac{1}{16}$ inch, and so on, for in each case the relation remains the same, viz. 1 : 72.

The object of making such tests was to get the best possible return for a given weight of steel. On the analogy of the voltaic cell, this object would be attained when the internal and external resistances were equal. With the proportion at 1 to 70 my tests showed that this equality practically existed; and, consequently, that the resistance of tungsten steel, as I use it, is about one seventieth that of air, and therefore about twenty times that of soft iron. The actual intensity obtained is also very high, being in the ordinary pattern of my meter nearly 4000 in the air-space, or at least half that in the air-space of a modern dynamo, and represents a magnetizing force of 1000 ampere-turns on a soft-iron core of full sectional area.

Some very simple, but practically important, results follow from the assumed analogy to the voltaic cell. In indicating them I neglect the resistance of the cast iron in the circuit, as this can always be made negligible at a trifling extra cost. Thus we can increase the intensity in an air-space of constant area in the proportion of the square root of the weight of steel magnets. We can, for instance, double the intensity by doubling the length, and at the same time doubling the cross-section of the magnet-bars, for then the magneto-motive force is doubled, and all resistances remain the same. Again, we may double the intensity in the air-space by

simply halving the area and halving the distance, for we shall then have made no change either in the magneto-motive force or the resistances, and, consequently, the same total induction is compressed into half the area ; or, again, we may double the distance between the pole-pieces and preserve the same intensity by simply doubling the length of the magnets. It is, in fact, quite easy to design an arrangement of permanent magnets that shall, within obvious limits, produce any given intensity of magnetic field in any given area of cross-section of air-space. These conclusions have been to a great extent tested by practice, and so far as they go the analogy on which they are founded holds good. The analogy is, in fact, *mutatis mutandis*, one of universal application.

The current by which the circuit is magnetized is always applied when everything is in position ; otherwise very small intensity results. The magnets are flashed by a current from a dynamo to practical saturation. The coils are then removed, and the magnets hammered severely. The hammering seldom reduces the intensity more than about one per cent. In months of continued testing I have never found any measurable falling-off in the field of a magnet so treated. If a reversing current is applied, the intensity will spring up on removal of the current, and will sometimes continue to rise appreciably for a considerable period. After a time it would probably fall again ; but, if the weakening under the reverse current has amounted to, say, twenty per cent., never to the lowest point then reached, and, perhaps, never to the point to which it springs at once, on the removal of the reverse current ; and, further, I have little doubt that an experiment might be arranged in which magnetizing and demagnetizing currents should be applied alternately in such a manner that, on their cessation, the successive strengthenings and weakenings of the field would repeat themselves backwards, just as was the case with the windings and unwindings in Sir W. Thomson's experiments on the "Fatigue of Metals." If so, the latest experiments on torsion would but add another analogy to Wiedemann's famous list.

XXI. *Note on M. Ph. Plantamour's Observations by means of Levels on the Periodic Movements of the Ground at Sècheron, near Geneva.* By CHARLES DAVISON, M.A., *Mathematical Master at King Edward's High School, Birmingham*.*

OBSERVATIONS on the movements of the ground by means of levels have recently been made by many

* Communicated by the Author.

astronomers and physicists in different parts of the world. But, at Sècheron, near Geneva, they have been conducted by M. Philippe Plantamour with an energy and perseverance which can find but few parallels, even in the annals of science. Since the end of 1878, without any important intermission, have his levels been read at least twice a day, and the results described in a series of valuable memoirs, published annually in the *Archives des Sciences physiques et naturelles**.

In this paper I propose to summarize the conclusions at which M. Plantamour has arrived: to show that a slightly different discussion of his observations will lead to a result of some interest; and to consider the cause or causes to which these movements may be due.

Arrangement of Levels, &c.—M. Plantamour's levels are placed in the cellar of his house at Sècheron, nearly two kilometres north of the observatory of Geneva, and 27 metres distant from the lake. The east front of the house faces the lake and is parallel to its shore-line, its direction being 30° E. of N. to 30° W. of S. The cellar, which is very dry, is excavated in compact blue glacial clay, covered by gravel on the west, north, and south sides of the house; and the ground on the west side is thus 2 metres higher than that on the east. M. Plantamour says that the subsoil of the house is 20 centimetres lower than the terrace; that is, I suppose, the clay beneath the floor of the cellar is at this depth below the ground on the east side of the house. Between the house and the lake the ground is nearly horizontal, the level of the lake being on an average one metre below the terrace. Behind the house the ground rises at an inclination of about 3 or 4 in 100 for about a kilometre, beyond which the slope increases more rapidly. In the immediate neighbourhood of the house, however, the slope of the land is considerable, and its direction cannot differ greatly from 30° S. of E. I give these details somewhat fully, as they have an important bearing on the results†.

* "Sur les déplacements de la bulle des niveaux à bulle d'air," *Arch. des Sc.* (July 1878) nouv. pèr. vol. lxiii. pp. 5-12; "Des mouvements périodiques du sol accusés par des niveaux à bulle d'air," *Arch. des Sc.*, 3^{me} pèr. (Dec. 1879), vol. ii. pp. 641-662; (Feb. 1881) vol. v. pp. 97-106; (Dec. 1881) vol. vi. pp. 601-610; (Dec. 1882) vol. viii. pp. 551-558; (Dec. 1883) vol. x. pp. 616-622; (Nov. 1884) vol. xii. pp. 388-394; (Nov. 1885) vol. xiv. pp. 443-448; (Dec. 1886) vol. xvi. pp. 566-571.

† The above account is taken partly from M. Plantamour's papers, partly from a letter to myself in which he describes the conditions at greater length. In a MS. sketch of the plan of his house, which he was good enough to send me, it would appear that the direction of the front of his house and of the shore-line of the lake is $17\frac{1}{2}^{\circ}$ E. of N. to $17\frac{1}{2}^{\circ}$ W. of S.; but I am not certain whether this plan is drawn exactly to scale:

The levels rest on a mass of masonry, built twenty years before the observations were begun, but *leaning against the outer west wall of the house*; they are at a depth of more than a metre below the ground outside, and at a distance of a metre from one another. M. Plantamour does not think that the movements indicated by the levels are due to oscillations of the outer wall under the action of changes of temperature. But we cannot be certain that they are not partly so caused, and it is impossible not to regret that observations undertaken with such care and perseverance should not have been made on levels completely isolated from all external accidents.

One of the levels is orientated east and west, the other north and south. During the first two years, however, owing to an error in the compass employed, their directions were 7° S. of E. to 7° N. of W., and 7° E. of N. to 7° W. of S., respectively. Readings were taken five times a day (at 9 A.M., noon, 3, 6, and 9 P.M.) during the first year, in order to determine the nature of the diurnal oscillations; and afterwards twice a day, at 9 A.M. and 6 P.M., that is, a little after the daily minimum and before the daily maximum. The mean of these readings is taken as the value of the inclination for the day.

Each year of observations begins on October 1, but, during the first year, the N.S. level was not ready until December 23, 1878. Tables are given in each memoir of the mean inclination of both levels for every day in the year, and the results are graphically shown on a plate, as well as the curve of the mean temperatures recorded at the observatory of Geneva.

Results of M. Plantamour's Observations.—1. In the E.W. level, a rise or fall of the external temperature produces a rise or fall, respectively, of the east end of the level: the movements of the ground lagging behind the changes of temperature. The extreme oscillations are determined by the continuity of a certain mean temperature rather than by the actual maxima or minima. The conclusion is therefore natural that variations of external temperature may be one of the principal causes of the periodic movements of the ground.

2. The period of oscillation experienced by the N.S. level is also annual, but its range is much smaller. In a general way, the south end follows the changes of external temperature, that is, it is lowered in winter and raised in summer. But the temperature variations of shorter period produce an inverse effect; as the temperature rises the south end is

lowered and *vice versâ*. This anomaly is never observed in the E.W. level, and its cause is unknown.

3. Diurnal variations are exhibited by both levels. In the E.W. level they are frequent; they vary much in intensity, being sometimes insensible, but at other times very pronounced, amounting to as much as $3''\cdot2$ (on Sept. 5, 1879). In the N.S. level they are rare and always small. When they occur in one level, moreover, they are absent in the other*. In the E.W. level M. E. Plantamour, applying Bessel's formula for periodic phenomena, finds that the maxima and minima of elevation of the east end generally fall between 6 and $7\frac{3}{4}$ P.M. and A.M.

4. Besides the periodic movements indicated by the E.W. level, there occurred during the second year of observations an extraordinary fall of the east end of the level, which it has never since recovered. On Nov. 13, 1879, the inclination was $+1''\cdot66$ towards the west. It fell gradually to $-5''\cdot80$ on Nov. 26, and then the downward movement became rapid, reaching $-88''\cdot71$ on Dec. 26. After this it rose slightly to $-82''\cdot16$ on Jan. 5, 1880; but again fell, attaining its maximum lowering of $-89''\cdot95$ on Jan. 28. During the following summer the east end rose no higher than $-74''\cdot05$ on Sept. 9. M. Plantamour attributes this extraordinary movement in part to the exceptionally low mean temperature of the winter of 1879-80; but, as he observes, there must be some other determining factor besides external temperature, whose cause is as yet undiscovered.

The following Table summarizes M. Plantamour's observations. In the last two columns I have added the average inclinations of both levels throughout the whole year† :—

* These features are readily explained if the annual and diurnal oscillations be due to the same cause. For then the resultant diurnal movement would take place in the same direction as the resultant annual movement at the same time, which is most frequently in an easterly, and but rarely in a northerly, direction. I am unable, however, to test this explanation, as M. Plantamour has only published the tables of diurnal motions for the E.W. level during the first year of observations.

† A + sign before a figure indicates a rise, and a — sign a fall, of the east and south ends of the two levels, with reference to their initial positions on Oct. 1 and Dec. 23, 1878, respectively. From Oct. 7-21, 1879, no readings were taken. In calculating the average inclinations for this year, 15 arithmetic means have been inserted between the figures for Oct. 6 and 22, namely, $+5''\cdot85$ and $+5''\cdot77$ for the E.W. level, and $-1''\cdot70$ and $-2''\cdot12$ for the N.S. level. Excluding these 15 days the average inclinations for the rest of the year would be $-70''\cdot87$ and $-2''\cdot63$. It should be added that in a few cases I have taken these figures from M.S. notes, not being able to re-consult the original papers.

Year.	Temperature.		E.W. Level. East End.				N.S. Level. South End.				Average Annual Inclination.	
	Max.	Min.	Date of		Inclination at		Date of		Inclination at		E.W. level.	N. level.
			Max. low.	Max. elev.	Max. low.	Max. elev.	Max. low.	Max. elev.	Max. low.	Max. elev.		
1878-79.	Dec. 24.	Aug. 3.	Jan. 15.	Sept. 8.	— 17'61	+ 10'47	Jan. 14.	Apr. 16, 17.	— 0'37	+ 3'82	— 6'84
1879-80.	Dec. 11.	July 19.	Jan. 28.	Sept. 9, 10.	— 89'95	— 74'05	Dec. 21.	June 2.	— 4'85	— 0'29	— 67'25	— 2'60
1880-81.	Jan. 23.	July 19.	Jan. 26.	July 15.	— 112'66	— 91'18	Jan. 26.	July 24.	— 9'83	+ 0'37	— 96'35	— 4'70
1881-82.	Dec. 29.	June 25.	Feb. 13.	Oct. 12, 1882.	— 112'07	— 88'93	Jan. 5, 24, 25.	June 14, 15.	— 7'30	+ 0'41	— 101'90	— 3'59
1882-83.	Jan. 25.	July 8.	March 15, 29.	Aug. 29.	— 112'29	— 92'53	Jan. 30.	July 17.	— 4'69	+ 1'87	— 101'42	— 1'78
1883-84.	Dec. 9.	July 14.	Apr. 24, 27, 29.	Sept. 21, 22.	— 117'12	— 97'73	Dec. 26.	July 28.	— 4'44	+ 2'74	— 108'01	— 0'88
1884-85.	Jan. 15.	Aug. 11.	Jan. 29.	Aug. 7.	— 128'16	— 103'16	Jan. 29.	Aug. 23.	— 3'86	+ 4'44	— 115'17	+ 0'90
1885-86.	Jan. 21.	July 22.	March 13, 14.	Sept. 6, 7, 8.	— 126'03	— 99'13	Jan. 22-24.	Aug. 1.	— 1'54	+ 7'64	— 113'34	+ 2'90

Curves of Resultant Motion.—It will be noticed from the preceding summary that M. Plantamour's discussion of his observations is confined to the components of the motion in two given directions, namely from east to west and from north to south. It may be possible, therefore, to learn something more from his observations by considering the movement which is the resultant of these two components.

The curves representing the resultant motion of the ground are drawn as follows :—

The zero of the E.W. level is its position on Oct. 1, 1878,

Fig. 1.



that of the N.S. level is its position on Dec. 23, 1878. The positive abscissæ and ordinates of points on the curve represent

the changes of inclination since these epochs towards the east and north respectively. The straight line joining any two points on the curve so obtained represents in magnitude and direction the resultant change of inclination during the corresponding interval. The direction of the longest axis of the curve is, therefore, that in which the greatest change of inclination has taken place.

In order to show the resultant change of inclination for every day in the year, the figures would, however, have to be inconveniently large. I have therefore made use of the average weekly changes of inclination only; except that the average is taken for a period of eight days at the end of the year of observations (Sept. 23–30), and also in the year 1884 in the interval including Feb. 29. The curves so obtained for the fourth to the eighth years of observation inclusive (1881–86) are given in fig. 1; the scale being such that one inch represents a change of 12'' in either direction. To avoid confusion the curves for different years are drawn below one another instead of being superposed.

Fig. 2 represents, on a scale ten times as great, the daily changes of inclination for the period corresponding to the right-hand portion of the curve for the seventh year (Dec. 24, 1884, to March 3, 1885). The dotted line, which represents, on the same enlarged scale, the average weekly changes of inclination during the same period, illustrates the simpler and average character of the latter curve.

From the curves in fig. 1, we may conclude:—(1) That the changes of inclination take place in different azimuths, showing that the component movements indicated by the two levels are not always in the same proportion to one another; (2) that, besides the yearly periodic movement, there is during the whole time illustrated a movement in a northerly direction, and, from the fifth to the seventh years, a considerable movement in an easterly direction as well; and (3) that the direction of the longer axis of all the curves is, on an average, about 12° S. of E, showing that *the greatest change of inclination during the year takes place approximately in the direction of the average slope of the surrounding ground.*

Connexion between the Changes of Temperature and the Movements of the Ground.—Let A B (fig. 3) represent a section of the surface of the ground (supposed plane), P, Q two points on the horizontal floor of the cellar.

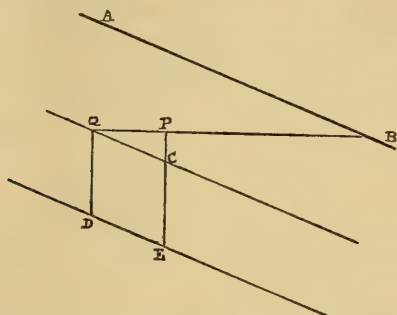
Let us suppose the external temperature to be rising. Let Q C represent the isothermal surface through Q, and D E that at which a given change of external temperature is just perceptible at the end of a certain time. If Q D, P C E be vertical

Fig. 2.



lines through Q and P, it is evident that the parts QD and CE will expand by the same amount after a given change of

Fig. 3.



external temperature ; and the rise of P relatively to Q will be due to the expansion of the part PC. Thus, as the temperature rises, the end P of a level PQ will rise.

As the temperature falls, on the other hand, the end P will fall after a short interval, and the bubble of the level will pass through its zero-point when the average temperature of PC is equal to its initial value.

Now M. Plantamour finds that, with the ground sloping in a direction nearly east, the rise and fall of the east end of the level follow after a short interval the rise and fall of the external temperature.

Again, the length of the line PC is greatest when the line PQ is perpendicular to the horizontal section of the isothermal surface through Q, that is, when the line PQ is parallel to the direction of the slope of the ground outside. Hence, the directions of greatest change of inclination and of the general slope of the ground should coincide.

Also, since the range of temperature due to a given change of external temperature diminishes as the depth below the surface of the ground increases, the change of level due to that change of external temperature must diminish as the distance from the ground increases. So that *two levels placed near one another in the same cellar, but at different distances from the surface, should indicate different variations of level.* And the variations may even be in opposite directions when the changes of external temperature take place in opposite directions.

Other conditions being the same, moreover, the extent of the movement varies as the tangent of the inclination of the land ; and this shows the desirability of choosing level ground for observatory-sites.

It remains to show that the cause invoked is sufficient, and yet not too great, to produce the effects observed. Let t° F. be the average rise of temperature over the distance P C (fig. 3), due to a given rise of external temperature; let $e = 7 \times 10^{-6}$; and let $\tan \alpha = \frac{1}{4}$, where α is the inclination of the ground outside. Then the value of t necessary to produce a change of inclination of $1''$ is given by the equation

$$t \times .000007 \times \frac{1}{4} PQ = \frac{PQ \times 22}{7 \times 180 \times 60 \times 60}$$

or

$$t = 2^{\circ}.8 \text{ nearly.}$$

The nearly uniform temperature of the cellar * will tend to increase this estimate; but, on the other hand, the linear coefficient of dilatation only has been used, whereas the expansion will take place chiefly upwards. Hence, as the levels are close to the surface of the ground, and as the annual and daily ranges of temperature at Geneva are considerable, we may, I think, conclude that *changes of external temperature are an extremely probable cause of the principal periodic movements of the ground observed by M. Plantamour.*

There are other movements, however, whose causes are not so clear. If the surface of the ground were perfectly plane, and the movements were due solely to changes of temperature, then we should expect the curves in fig. 1 to be simply straight lines parallel to the slope of the ground. Possibly the movements at right angles to this direction may be due to inequalities in the ground or to differences in its covering, causing the normal to the horizontal section of the isothermal surface at any point to vary. It is evident that the direction of QP (fig. 3), for which the product of the length PC into its change of average temperature is greatest, may vary, and the direction of the resultant motion be consequently changed.

Influence of the Formation and Melting of Polar Ice.—It may be thought that the alternate formation and melting of ice and snow on the two polar ice-caps might produce a small periodic movement in the direction of the meridian. It must, however, be inappreciable. For the centre of gravity of the earth must be displaced by 147 feet along its axis to produce a change of $1''$ in the direction of the vertical at Geneva. Let us suppose a layer of ice (of density .92) of uniform thickness to be melted off the whole south polar zone, and the

* It should be noted that Colonel Orff, who has made observations on levels in the cellar of the Observatory of Bogenhausen, near Munich, placed thermometers on the opposite sides of the pillars supporting the levels, the bulbs touching the pavement and being covered with sand. In one case the average difference of temperatures during the year amounted to $0^{\circ}.48$ C. (*Arch. des Sc.* 3^{me} pér. vol. vi. p. 618.)

material (to take an extreme case) to be entirely transferred to the north polar zone, and re-formed there as a sheet of ice of uniform thickness. Then, to cause such a displacement of the centre of gravity, the sheet melted from one zone and collected on the other would have to be more than 3800 feet in thickness. By such a transference of the material, the centre of gravity would indeed be still further displaced in consequence of the displacement of the ocean-waters*. But, attributing the greatest possible value to such a cause, it is evidently insufficient to produce a deviation of the vertical of $\cdot 04$ of a second, which is the smallest change of inclination perceptible in M. Plantamour's N.S. level.

Non-Periodic Movements.—To the geologist, perhaps, the most interesting of the movements observed by M. Plantamour will be those which seem as yet to be non-periodic in their occurrence. It will be seen from fig. 1 that these movements are not confined to the winter of 1879–80. The last two columns of the Table, which give the coordinates of what may be called the centre of the year, show also that this centre undergoes a displacement greater than can be accounted for by mere differences of temperature from one year to another. The extraordinary movement of the second year is continued, though to a less extent, during the third year, and the centre is again shifted from the fifth to the sixth and from the sixth to the seventh year. From the third to the eighth years inclusive the centre has moved constantly towards the north. It is not unlikely, of course, that these movements may be local or accidental in their origin, and independent of any secular or wide-spread action. But it is at least possible that they are due to great earth-movements: that they represent, for our finite time, the infinitesimal changes which culminate in a great mountain-chain.

To observe these movements with a greater chance of success it would be advisable to separate them from the strictly periodic movements caused by the annual and diurnal changes of external temperature. The latter diminish as the distance from the surface of the ground increases, and must be insensible at a depth of about 60 or 70 feet. At this, or a greater, depth levels might be observed in a deserted or unfrequented mine; and they need not, for this purpose, be read oftener than once a week. If this were done at two or three places on either side of a recently-formed mountain-chain, like the Apennines or Alps, the results could hardly fail to throw light on some of the great problems in the theory of terrestrial evolution.

* See Dr. Croll's 'Climate and Time.'

XXII. *Notices respecting New Books.*

A Treatise on Electricity and Magnetism. By E. MASCAET and J. JOUBERT. Translated by E. ATKINSON, Ph.D., F.C.S.

Vol. II. *Methods of Measurement and Applications.*

IT is hardly necessary to say that MM. Mascart and Joubert have, in publishing this treatise, done a very great service to Electrical Students. Those who are not acquainted with the work will obtain a very good idea of its nature if they imagine that a professor well acquainted with Clerk-Maxwell's work has here tried to give to students equipped only with the usual knowledge of the Differential and Integral Calculus, not only a clear understanding of Maxwell, but a clear knowledge of what has been done in developing the quantitative study of electricity since Maxwell's time. This second volume is likely to be of very great use to experimenters and electrical engineers. The treatment of the subject is very complete; indeed it strikes a person who reads steadily through the book that there is somewhat too much detail; but it is very probable that when the same reader uses the book afterwards as a book of reference, he may feel very thankful for the elaborate working out of corrections which are of so much importance in the laboratory. We are not now speaking of such details as the proof of the law of torsion in a cylindric wire (pages 55-6), because such matter seems to us as unnecessary as the proof of one of Euclid's propositions in such a book. Again, there are certain refinements in correction which seem to us unnecessary; for example, the length of one spire in the winding of a coil may usually be taken as equal to the circumference of the axial cylinder without the introduction of a correcting term. It is, however, quite probable that other readers may not agree with us here; whereas as to the obvious great merits of the book, and even to such a merit in the translation as the creation of an index (although the index might be advantageously amplified), there can be no difference of opinion.

The work is divided into four parts:—I. Methods of Measurement, 165 pages. II. Electrical Measurements, 400 pages. III. Magnetic Measurements, 111 pages. IV. Complement, 88 pages.

We shall say a few words about Part I., for the purpose of giving an idea of the general method of treatment adopted by the authors and translator.

Part I. begins with corrections for temperature in measuring *length*; corrections for the air in weighing; and 13 pages are devoted to a description of various methods of measuring angles and their limits of accuracy, the methods described being all optical. The second chapter is devoted to a study of vibrations; vibrations damped when the resistance is proportional to the velocity, and also when it is proportional to the square of the velocity. This is a chapter which will be found very useful in the laboratory; and

the end part is particularly important in magnetic work ; although we must confess that here, as in the rest of the book, if the translation were less literal, and were slightly altered in places by the introduction of English technical terms instead of the literal translations of French terms, it would be more useful to students.

The third chapter, on the Measurement of Couples, is rightly very elaborate, but in our opinion the authors are a little weak in regard to the properties of materials ; they do not sufficiently understand the importance of "Fatigue" in materials subjected to strains. In § 712 they neglect the expansion of the vibrating body. In § 713 they do not seem to be aware that, if silver wire were drawn out very fine, its tenacity per square centimetre and its coefficient of rigidity might be very greatly altered.

Chapter iv. treats of the properties of Circular Currents. Twenty pages are devoted to the magnetic moment of a cylindric coil and the field at the middle of its axis, leading to well-known theorems concerning the most suitable dimensions of the coil for a given length of wire ; or to give greatest field when the source of the current is given ; or the best radial section of coil of a galvanometer ; in these various cases the section of the wire may be constant or varying. In places (§ 734 for example) the temptation to pursue a mathematical investigation to the end, although the result may be of but little practical importance, has not been perhaps sufficiently resisted. The twenty-four remaining pages of the chapter are devoted to finding useful expressions for the field at other points than the central one, and these naturally lead to the expressions in chapter v. for the coefficients of induction of coils. The field at any point due to a circular current is obtained in a series involving the powers of the coordinates x and y of the point, and the series are integrated to obtain the field for a cylindrical coil ; it being shown that only certain terms of the series need be taken in particular cases, the Quadrature method and Maxwell's "mean-value" method being given for a long coil. The results are applied to find the action of the coil on a magnetic needle ; to show the value of Gaugain's method of placing the needle ; the value of Helmholtz's arrangement of two parallel coils ; the three parallel coils ; the four parallel coils ; and also two parallel coils with currents in opposite directions. Knowing the field at a point, there is a general rule for finding the average field [the *mean action* it is called in this treatise] at points on any circular area whose plane is at right angles to the axis. Finding this average field in the case of an annular coil (like the winding on a Gramme ring) for portions of the cross section is evidently given by the authors as a mathematical exercise ; of course the results are of no practical importance.

In chapter v., on coefficients of induction, the results of chap. iv. are at once applicable in any case where the size of the wire is negligible. But the method of expressing the coefficient of mutual induction of two spires near one another by elliptic integrals with Maxwell's useful table is also given, and its extension to coils with

the same axis. The entirely different method of taking up the subjects of chapters iv. and v., namely by Spherical Harmonics, or rather by Zonal Harmonics only, is now entered upon, and we could have wished that the whole treatment of the subject had been on this method. We know of no mathematical subject, capable of being of great value to the practical electrician, which has received so little attention as this of Spherical Harmonics. We know of no publication, for example, of the general development in Spherical Harmonics of the magnetic potential due to a current flowing in a cylindric coil such as these authors are considering; and it is not at all easy at first sight to obtain it. When it is obtained the practical man will find it quite useless, as, near the end of the coil, he will require to make his calculation from about twenty terms. When some practical mathematician takes up this subject seriously, he will approximately give the potential by means of a series of a few terms only, and show the working electrician that the subject of Spherical Harmonics is not merely a beautiful mathematical conception, but that it can really be made of use; and when that time arrives, men will have as definite notions concerning the attractions of coils for one another, and the values of the coefficients of self and mutual induction of coils as they have of resistance. At the present time few electricians would be surprised to hear that their notions of the values of these magnitudes for particular coils were 100 times too great or too little. Although, however, we might wish a fuller treatment of the subjects of these two chapters by Spherical Harmonics, readers will find that these subjects are here entered into much more fully than in any other treatise. The potential on the axis due to a current in a circular spire; the external potential of a long coil; the mutual induction of two long coils; the potential of coils wound spherically; mutual induction of circular currents; these are investigations taken up by the Spherical Harmonic method. Maxwell's method of obtaining the Coefficient of Self-Induction of a coil is carefully worked out, and the chapter finishes with the study of some particular cases. At the beginning of chapter v. we find taken up the case of parallel currents when the size of the wires cannot be neglected.

It would be quite easy from our notes to describe in the same sort of way the rest of this volume, but readers will find that our description of Part I. applies fairly well to the whole volume. There is everywhere else the same careful development of details which Maxwell and others have left somewhat obscure; and it is evident that, in assisting students so carefully as they have done, the authors were engaged in a labour which delighted them.

We are sorry to say that it will be necessary to make a number of corrections of printers' errors in a second edition. These errors put great difficulties in the way of students; for it has to be borne in mind that many of the letters in use by French mathematicians are such as would not be used in English books. α represents an angle, for example, where we should use θ ; T is the time of only half what we call a complete oscillation. A weight in grammes is

p instead of w . The radius of a wire is y , the specific gravity of a metal is d . The letter p is also used for a number of layers of wire. Instead of *log.* here l is conveniently used, but l is also used to indicate a *length*.

After all, these do not delay the progress of the earnest student. It is only when the printer uses l and \bar{l} indifferently for *log.*, and mingles his letters curiously in mathematical formulæ, and leaves out very important half-brackets, and perhaps in the same page uses l to represent also a length, that the student has a right to complain, and it is only fair to say that the printers' errors in this volume are much too numerous.

We note that the authors are careful in almost every case to give the names of the various scientific men whose work they make use of. One noticeable exception is the name of Prof. Tait, in the description of his beautiful geometrical treatment of the damping of vibrations.

Although we have said that the book will benefit by some revision, we think that Dr. Atkinson, in undertaking and carrying out this translation, has done a very great service to, and will receive the thanks of, a very considerable number of electricians.

XXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 78.]

December 5, 1888.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. "Notes on two Traverses of the Crystalline Rocks of the Alps." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

These journeys were undertaken in the summer of 1887, in the company of the Rev. E. Hill, F.G.S., in order to ascertain whether the apparent stratigraphical succession among the gneisses and crystalline schists which the author had observed in the more central region of the Alps, held good also in the Western and Eastern Alps. At the same time all circumstances which seemed to throw any light on the origin of the schists were carefully noted. The author examined the rocks along two lines of section:—(1) By the road of the Col du Lautaret from Grenoble to Briançon, and thence by the Mont Genève and the Col de Sestrières to Pinerolo, on the margin of the plain of Piedmont. (2) From Lienz, on the upper waters of the Drave, to Kitzbuhel; besides examining other parts of the central range, east of the Brenner Pass. The specimens collected have subsequently been examined microscopically.

The results of the author's investigation may be briefly summarized as follows:—

(1) While rocks of igneous origin occur at all horizons among the crystalline series of the Alps, these, as a rule, can be distinguished; or, at any rate, even if the crystalline schists in some cases are

only modified igneous rocks, these are associated with recognizable igneous rocks of later date.

(2) There are, speaking in general terms, three great rock-groups in the Alps which simulate curiously, if they do not indicate stratigraphical sequence. The lowest and oldest resembles the gneisses of the Laurentian series; the next, those rather "friable" gneisses and schists called by Dr. Sterry Hunt the Montalban series; the third and uppermost is a great group of schists, generally rather fine-grained, micaceous, chloritic, epidotic, calcareous and quartzose, passing occasionally into crystalline limestones, and (more rarely) into schistose quartzites.

(3) The Pietra Verde group of Dr. Sterry Hunt, so far as the author has been able to ascertain, consists mainly of modified igneous rocks, of indeterminable date, and is at most only of local, if, indeed, it be of any classificatory value.

(4) Of the above three groups the uppermost has an immense development in the Italian Alps and in the Tyrol, north and south of the central range. It can, in fact, be traced, apparently at the top of the crystalline succession, from one end of the Alpine chain to the other.

(5) The middle group is not seldom either imperfectly developed or even wanting, appearing as if cut out by denudation. It was not seen in the traverse of the Franco-Italian Alps, except perhaps for a comparatively short distance on the eastern side, being probably concealed by Palæozoic and Mesozoic rocks on the western side. It is not very completely developed in the Eastern Tyrol, and seems to prevail especially in the Lepontine Alps, and on the southern side of the watershed.

(6) The lowest group is fairly well exposed, both in the French Alps and in the Central Tyrol.

(7) As a rule, the schists of the uppermost group had a sedimentary origin. The schists and gneisses of the middle group very probably, in part at least, had a similar origin. In regard to the lowest group it is difficult, in the present state of our knowledge, to come to any conclusion.

(8) The slates and other rocks of clastic origin in the Alps, whether of Mesozoic or of Palæozoic age, though somewhat modified by pressure, are totally distinct from the true schists above mentioned, and it is only under very exceptional circumstances, and in very restricted areas, that there is the slightest difficulty in distinguishing between them. The evidence of the coarser fragmental material in these Palæozoic and later rocks indicates that the gneisses and crystalline schists of the Alps are very much more ancient than even the oldest of them.

(9) The remarks made by the author in his Presidential Address, 1886, as to the existence of a "cleavage-foliation" due to pressure, and a "stratification-foliation" of earlier date, which seemingly is the result of an original bedding, and as to the importance of distinguishing these structures (generally not a difficult thing), have been most fully confirmed. He is convinced that many of the con-

tradictory statements and much of the confusion in regard to the origin and significance of foliation are due to the failure to recognize the distinctness of these two structures. In regard to them it may be admitted that sometimes "extremes meet," and a crystalline rock pulverized *in situ* is very difficult to separate from a greatly squeezed fine-grained sediment; but he believes these difficulties to be very local, probably only of a temporary character, and of little value for inductive purposes.

2. "On Fulgurites from Monte Viso." By Frank Rutley, Esq., F.G.S., Lecturer on Mineralogy in the Royal School of Mines.

The specimens described in this paper were collected by Mr. James Eccles, F.G.S., close to the summit of Monte Viso (12,680 feet above sea-level). They are fragments of a glaucophane-epidote schist, in which garnet, sphene, and occasionally diallage are present. Prof. Judd considers that the rock somewhat closely resembles the glaucophane schists and eclogites of the Ile de Groix.

The fragments are bounded by joint-planes or surfaces of easy fission, which are incrustated with minute pellets and thin films of fulgurite-glass forming the walls of lightning tubes. The glass was examined under the microscope (great care being taken to insure perfect isolation of the glass from the rest of the rock), and found to be, as a rule, remarkably pure, but in places not only gas-bubbles but also globulites occur, and the latter occasionally form longulites, and more rarely margarites. Microliths also are observable in some of the sections. In one section a minute rounded grain of schist containing a fragment of a strongly depolarizing crystal, probably epidote, appears to have been taken up in the glass.

Where the glass comes in contact with the rock the latter appears to have undergone no alteration beyond the development of a very narrow band of opaque white matter, which the author gave reasons for supposing to be due, not to the action of the lightning, but to a pre-existent segregation of sphene.

The occurrence of globulites, margarites, longulites, and microliths in the glass would seem to indicate a less sudden cooling than is assumed to be usual in such cases; for the glass presents no signs which would characterize a subsequent devitrification or secondary change, and the bodies just enumerated appear, unquestionably, to have been formed during the refrigeration of the fulgurite.

3. "On the occurrence of a new form of Tachylite in association with the Gabbro of Carrock Fell, in the Lake District." By T. T. Groom, Esq.

In this paper the author described an ancient but well-preserved glassy rock of basic composition which he had found as a vein associated with the gabbro of Carrock Fell. The rock was described macroscopically and microscopically, and a complete chemical analysis was given. The chemical composition resembled that of the more acid basalts and the augite-andesites, and approached especially closely to some continental basalts, analyses of which were added for comparison. Examined microscopically, the rock consisted of a

globulitic and crystallitic glass-basis of green colour, containing spherules of quartz, spherulitic feldspars, and an interesting series of granules and granular aggregates of augite, which likewise frequently assumed a spherulitic form. The rock was rendered microporphyritic by the sparing development of crystals (or skeleton-crystals) of plagioclase feldspar, augite and quartz. The optical characters of each of the minerals were given. Owing to the mode of development and to the variety of its constituents, the rock possessed an exceedingly complicated structure. The order of crystallization was worked out, and it was pointed out that a second generation of each of the important constituents had arisen. The second generation of feldspar was of a more acid type, and that of the quartz was devoid of fluid vesicles, and had crystallized out after the rest of the rock had solidified sufficiently to form cracks. Close physical and mineralogical relations with the gabbro were indicated, and the author had no doubt that the two were in actual connexion with one another. The age was put down as probably Ordovician. A comparison of the rock with other basic rocks showed that it had affinities both with the glassy forms connected with the more volcanic members, and with the variolites of Durance found associated with diabase. The relation with the latter rock was especially marked, but important points of difference rendered a separation of the two necessary, and for the new type of rock thus recognized the name *Carrockite* was suggested. This rock might be looked upon as a *Quartz-Gabbro-Vitrophyre*.

XXIV. *Intelligence and Miscellaneous Articles.*

ON THE EFFECT OF TWIST IN MAGNETIZATION.

BY MR. H. NAGAOKA.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a private letter to me, dated November 11, 1888, Mr. Nagaoka describes his further researches into the effect of twist in magnetization. His special object is to study the effect of different *ranges* of twist (*i. e.* the maximum twist to which the cycle of twisting is carried).

He finds that in iron the essential features of the curve of twist and magnetism remain unchanged irrespective of the intensities of either the magnetizing-field or the longitudinal stress. This confirms the results stated by Mr. Bottomley and myself in the latter part of our note. When the range of twist is increased beyond $\pm 11^\circ$ per centim. (a very large amount of twist), although the general form of the curve remains similar, the hysteresis produced by the cycle of twisting and untwisting becomes reversed; that is to say, the untwisting part of the curve lies above the twisting part, instead of being below, as it is when the range of twist is moderate. In nickel the effect of range is more curious and interesting; but to describe even all that is in the letter will require more space than I would dare ask in such a note as this. I may

add that we may anticipate the publication of Mr. Nagaoka's further work in the Journal of the College of Science, Imperial University, Japan, in the near future.

I am, Sir,

Yours faithfully,

Glasgow University Jan. 21, 1889.

AIKITU TANAKADATÉ.

ON THE ALTERATION OF THE CONSTANT OF ELASTICITY OF METALS BY THE ELECTRICAL CURRENT. BY C. A. MEBIUS.

From his investigations on the elasticity of metals, Wertheim concluded that an electrical current, which traverses a metal wire, diminishes the coefficient of elasticity. The determinations of Wertheim are not very trustworthy, and from recent experiments Streintz has questioned the conclusions at which Wertheim arrived.

Mebius has again taken up the question and attempted to solve it by experiments on flexure. The materials under investigation (two rods of steel and two of iron, two tubes of brass, one silver wire) were laid on two parallel horizontal knife-edges, and the centre was loaded. By means of a microscope with micrometer in the eyepiece, an index could be sighted. The author could not observe any direct action of the current on the bending. From a discussion of the results of the observations, it follows that in four experiments, if there had been any alteration of the coefficient of elasticity, it would have amounted to less than 0.089, 0.015, 0.047, and 0.037 per cent. of its entire amount, and accordingly, in agreement with Streintz, he assumes that the electrical current has no action on the elasticity.—*Eftvers. af k. Vet.-Akad. Förhandl.* 1887, p. 681; *Beiblätter der Physik*, vol. xii. p. 678 (1888).

ON AN ELECTROMETER WITH A QUARTZ DOUBLE PLATE.

BY M. JACQUES AND P. CURIE.

Two rectangular plates are cut at right angles to the axis so that their long sides are simultaneously at right angles to the optical and electrical axis. The plates are very thin, cut to a few hundredths of a millimetre, and are then cemented to each other, so that the electrical axes in them are reversed. The two external faces are silvered, and freed from the silvering on a narrow edge. If the silver coatings are brought to a difference of potential, the plate curves, as can be seen at a glass index placed at the free end of the plate. At its end a small photographed micrometer is fastened and is viewed by a microscope.

If $k = 6.32 \times 10^{-8}$, the deflexion θ of the end of the needle $= \frac{3}{2} k (L/e)^2 \cdot (2\lambda + L)/L \cdot V$ in absolute electrostatic measure (C.G.S.), where L = the length of the quartz plate, e the thickness, λ the length of the needle, V the difference in potential in absolute measure (that is, a unit equals 290 volts).

The instrument is aperiodic, but not very sensitive, and can hence be used for higher potentials, that is from about 0.5 to several thousand volts.—*Comptes Rendus*, p. 1287 (1888); *Beiblätter der Physik*, vol. xii. p. 687.

EFFECTS OF LIGHTNING.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Mr. Dudgeon, of Cargen, Dumfries, who supplied me with an interesting case of an upward discharge of lightning which occurred in June last, and is described in my communication to you in August, was attracted by one of the cases given in my paper of December (p. 483), in which a girl at Geneva is described as being literally wrapped in a sheet of electric fire. Mr. Dudgeon states that a similar phenomenon occurred to a friend of his during a severe thunderstorm, which burst over a large tract in the south of Scotland on the 12th of August, 1884—the storm in which Lord Lauderdale was killed when out shooting in Roxburghshire. Mr. Dudgeon says:—“My friend Sir Alexander Jardine was out on the moors on the same day. He felt the full effects of the storm before the phenomenon I am about to describe took place. Both he and his gamekeeper got quite wet from heavy rain, and the rain ceased when the thunderstorm was felt most severely. As Sir Alexander described it to me, he was enveloped in a sheet of light which seemed to him like illuminated steam. He does not remember noticing any pricking sensation, and on my asking him what the feeling was, he said it was indescribable, but a more uncomfortable and weird feeling he never experienced. The gamekeeper, who was standing about ten yards from him, saw him enveloped in the luminous cloud, and exclaimed when it cleared, ‘Oh! Sir Alexander, I never thought to see you again!’ Sir Alexander went home, having received a severe shock, which brought on a violent headache. He went to bed, and felt the effects for some time afterwards. Jardine Hall, near which the occurrence took place, is about 12 miles N.E. from Dumfries.”

Highgate, N., 12th January, 1889.

I am, &c., C. TOMLINSON.

THE BRESSA PRIZE.

The Royal Academy of Sciences of Turin gives notice that from the 1st of January, 1887, the new term for competition for the seventh Bressa Prize has begun, to which, according to the testator's will, scientific men and inventors of all nations will be admitted. A prize will therefore be given to the scientific author or inventor, whatever be his nationality, who during the years 1887–90, “according to the judgment of the Royal Academy of Sciences of Turin, shall have made the most important and useful discovery, or published the most valuable work on physical and experimental science, natural history, mathematics, chemistry, physiology, and pathology, as well as geology, history, geography, and statistics.”

The term will be closed at the end of December 1890.

The value of the prize amounts to 12,000 Italian lire.

The prize will in no case be given to any of the National Members of the Academy of Turin, resident or non-resident.

The President of the R. Academy,

Turin, January 1st, 1889.

A. GENOCCHI.

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[FIFTH SERIES.]

MARCH 1889.

XXV. *On the Divergence of Electromotive Forces from Thermochemical Data.* By E. F. HERROUN, *Professor of Natural Philosophy in Queen's College, and Demonstrator in King's College, London*.*

THE fact that with many voltaic cells the electromotive forces observed experimentally do not accord with values calculated from the thermochemical equations representing the reactions occurring within the cells, has been the subject of remark by several investigators, including Favre, Julius Thomsen, Braun, Helmholtz, Willard Gibbs, Wright and Thompson, and others. But although, at the present time, we are in possession of a knowledge of the actual and computed values of the electromotive forces of a large number of voltaic cells, anything approaching a complete explanation of the reason why certain cells should give electromotive forces in excess, others in defect, others again according almost exactly with the calculable values, is still wanting.

It was in the hope of deciding certain questions and of going some way on the road to a final solution of the problem that the present research was undertaken.

Certain cells, like the 'Grove' or 'Bunsen,' in which nitric acid is reduced by the evolved hydrogen, are said to furnish, with nitric acid of certain degrees of concentration, electromotive forces somewhat in excess of the values calculated from the thermal effects accompanying the chemical changes;

* Communicated by the Physical Society: read January 26, 1889.

but the effect of solution of the oxides of nitrogen or their evolution as gas, together with other effects of dilution of the acid, complicate the subject from the theoretical standpoint, and their non-reversibility renders them unsuited for experiment. Cells of the Daniell type are therefore greatly to be preferred, both for theoretical consideration and direct experiment.

Thanks to the careful and accurate data furnished by the published researches of Dr. C. R. A. Wright and Mr. C. Thompson, and their clearly tabulated results*, one can see at a glance whether the electromotive force furnished by two metals immersed in solutions of their corresponding salts accords with, exceeds, or falls below the value calculated from thermochemical data, and the exact amount of the discrepancy.

The explanations which might be given of the cause of the departure of observed E.M.F.s from the calculated values in different cells may be classified under the following heads:—

1. Certain cells in which the anticipated chemical change does not occur, but some other, different, reaction which evolves heat, to an extent which would account for the observed E.M.F. This is brought about by the special behaviour of a metal surface, and probably depends upon—

2. The coating of a metal surface by films of oxide or subsalts either more or less readily reduced than the normal salt of that metal.

3. The effect of dissolved gases, notably oxygen, in the solutions of the metallic salts.

4. The hydration or solution of the salts formed in some cases, but not in all, supplementing or diminishing the E.M.F. due to the reaction of the solid salts.

5. The evolution or absorption of sensible heat in the cell, and the loss or gain in electric energy, corresponding thereto.

In illustration of case 1 may be mentioned a cell consisting of aluminium in aluminic sulphate opposed to zinc in zinc sulphate, in which the thermochemical data would indicate that aluminium would be the metal attacked, giving, when opposed to zinc, an E.M.F. of about 1 volt; whereas, in practice, zinc is the metal which is attacked, and the cell furnishes an E.M.F. of about .53 volt. But we know that the chemical reaction $\text{Al}_2(\text{SO}_4)_3 + \text{Zn}_3 = 3(\text{ZnSO}_4) + \text{Al}_2$ does

* "The Determination of Chemical Affinity in terms of Electromotive Force," pt. ix., Proc. Phys. Soc. vol. vi. pt. iv., and Phil. Mag. ser. 5, vol. xix. No. 118.

not occur, so that the aluminium plate merely acts as an imperfect substitute for an entirely non-attackable substance such as platinum or carbon, and having *hydrogen*, not aluminium, deposited on its surface.

That this is the case, and that the reason is probably to be found in the superficial coating of the aluminium plate by an oxide or sesquioxide, has been already put forward and received experimental verification by Dr. A. P. Laurie*, who has investigated the effect of amalgamating the aluminium, which better enables it to come in actual contact with the liquid and causes the E.M.F. of the cell to actually reverse in direction.

The heat developed by the oxidation or conversion into sulphate of the zinc with the liberation of hydrogen would be more than sufficient to account for the observed electromotive force; hence if it be desired to trace any connexion between the absorption or evolution of heat and the anomalous E.M.F. of any cell, it is the actual chemical change that does occur which must be made the basis of the calculation. Further, the term "*Thermovoltaiic Constant*" (employed by Messrs. Wright and Thompson to denote the extent of departure of the E.M.F. of a cell from thermochemical data; zinc being taken as the standard from which the voltage is reckoned) in cases such as aluminium can merely express the fact that a plate of aluminium in its compact form, and probably covered by its protecting film of oxide, is less actively attacked than it ought to be from theoretical consideration of the heat of formation of its salts; but it must not be accepted as denoting any occult power of transforming electric energy into heat or *vice versâ*.

From purely *à priori* considerations one would expect that metals like aluminium and magnesium, which in the amalgamated or finely divided condition readily decompose water, would, in the compact form, become covered superficially with oxide and behave to a greater or less extent as a hydrogen plate. It is therefore largely to this cause that one would look for an explanation of the large + values assigned to the "*thermovoltaiic constants*" of aluminium, magnesium, and possibly iron. The observation by Wright and Thompson, that the substitution of dilute sulphuric acid for ferrous sulphate as the liquid surrounding the iron plate of an iron-copper cell uniformly tends to *lower* the E.M.F., which effect they refer to "*local action*," distinctly supports the view that even with iron there is a tendency to decompose water and coat itself with hydrogen, and thus acquire a higher relative potential.

* Phil. Mag. ser. 5, vol. xxii. p. 213.

A further consideration of case 2 will be met with later in this paper. I will merely mention that a considerable difference in E.M.F. due to the superficial coating of a copper plate with suboxide has been already noticed in the use of standard Daniell cells by Dr. J. A. Fleming* and myself†, and that what is rare and comparatively unimportant in the case of copper may become the rule and of greater importance in some other combinations.

In case 3 the nature of the effects of dissolved oxygen in oxidizing a metallic surface or in removing traces of electrolytic hydrogen and its depolarizing effect in the latter case are too obvious to need much comment; only I would draw attention to the fact that by its action the electromotive force on open circuit, as measured by an electrometer, or with a balance of difference of potential, as in Latimer Clark's method, or even when measured by a galvanometer of many thousand ohms resistance, is liable to be in excess of that due to the supposed chemical reaction, and therefore to give false ideas as to the energy-producing power of the cell.

Case 4 has been suggested as a very probable source of divergence, since (1) in cells in which a very sparingly soluble salt is employed, the solution, especially in the vicinity of the electrode, may become saturated, and if any more salt be formed it will necessarily be produced in the solid form, which may, or may not, be dissolved in the rest of the liquid; (2) it is difficult to see how the tendency of a salt to combine molecularly with water to form a hydrate or to dissolve as a solution, can directly affect the combination of the metal with the acid radical to form that salt, and therefore we may be justified in concluding that both the total heat evolved and the E.M.F. of a cell are due to the algebraic sum of effects of events which take place successively. But since the water in different solutions is differently combined or related with the anhydrous salt, it appears possible that the energy which in some cases is transformable into electric energy, or, to use Helmholtz's suggestive names, is "free," in other cases may be simply "bound energy."

The main interest of the question, however, centres in case 5, since many physicists seem to have tacitly assumed that the explanation of the divergence of actual E.M.F.s from the computed values is simply that with cells giving a deficiency of electromotive force a portion of the chemical energy appears as heat in the cell, while cells giving an excessive

* Phil. Mag. ser. 5, vol. xx. p. 132; Proc. Phys. Soc. vol. vii. p. 168.

† Phil. Mag. ser. 5, vol. xxi. p. 12; Proc. Phys. Soc. vol. vii. p. 276.

electromotive force are spoken of as doing so "at the expense of sensible heat." They have not, however, given any reason for such differences of action, and they do not appear to have considered all the consequences which the adoption of the latter part of this view would entail.

Helmholtz, in his papers "*Zur Thermodynamik chemischer Vorgänge*,"* suggests the treatment of a voltaic cell as a reversible thermodynamical process, and that the nature of its working may be detected by the difference of E.M.F. brought about by change of temperature. The cell selected for examination is his zinc-mercurous chloride battery, the E.M.F. of which is very slightly increased by rise of temperature; and the chief discussion is the relation of the vapour-tension of the zinc-chloride solution, the "free energy of the salts," and the electromotive force dependent on differences of concentration.

But that part of his conclusion in which he states that "*the electromotive force between the metals increases with heating, i. e. the calomel battery belongs, as I have already mentioned, to the batteries which fix heat, which in part work at the expense of the thermometric heat of surrounding bodies*," is based either purely on thermochemical data or without due consideration of what the adoption of that view would entail in the case of the strictly comparable zinc-mercurous sulphate cell of Latimer Clark, which decreases in E.M.F. by a considerable proportion as its temperature is raised, although this also is a cell giving an E.M.F. in excess of the supposed thermochemical effect.

Now viewing a cell as a strictly reversible thermic engine, if a portion of its E.M.F. be due to the absorption of sensible heat, then, when worked backwards, an exactly equivalent amount of heat would be evolved in the cell. Therefore, taking two Latimer Clark cells, joined by their like poles, one at a high, the other at a low relative temperature, the one at the low temperature having the higher E.M.F. will send a current against the E.M.F. of the one at the higher temperature.

But the current through the cold one will be direct, while that through the warm one will be inverse, so that heat will be absorbed in the cell at low temperature and evolved in the cell at high temperature; and since for unit quantity of electricity flowing the same amount of chemical work will be performed in each cell, but reversed in one, we should have an instance of a thermodynamical arrangement working by means

* *Sitzungsbericht. d. Akad. d. Wissenschaft. zu Berlin.* Translated and pub. in *Physical Memoirs*, by the Phys. Society, vol. i. pt. 1.

of a difference of temperature, the effect of which would be to cool the cold cell and heat the hot one ; or heat would be transferred from the cold to the hot body, and be made capable of performing mechanical work, instead of requiring its expenditure. It does not appear to me that this could be adequately explained by the different heats of dilution of the salt at different temperatures, so that the result of believing that a Latimer Clark cell works partly "at the expense of sensible heat" is a direct contradiction of the principle of the second law of thermodynamics, as this would require that its E.M.F. should rise with rise of temperature. That it is the postulate with regard to mercury cells, and not the second law, which is fallacious, I hope to show later in this paper.

If two plates of zinc, one amalgamated and the other plain, be immersed in a solution of zinc sulphate, on completing the external circuit a current is found to flow from the amalgamated to the non-amalgamated plate through the cell ; but Prof. J. Willard Gibbs in his paper "On the Equilibrium of Heterogeneous Substances,"* remarks that such a current could not have the effect of dissolving zinc from the amalgamated plate and depositing it on the non-amalgamated plate, as this deposited zinc might be immediately redissolved in mercury to form an amalgam with merely the absorption of sensible heat.

By the use of pure zinc foil in solutions of zinc sulphate, free from oxygen, I have found that the difference of potential between amalgamated and plain zinc becomes exceedingly small ; and in the case of a cell freely exposed to air the current on a circuit of only 1.5 ohm's resistance did not deposit any weighable quantity of zinc on the plain zinc electrode after passing for seven days. This, therefore, is in confirmation of the theoretical conclusion from the second law put forward by Willard Gibbs, and renders it more imperative to stringently examine any reputed case of the production of electrical energy at the expense of thermometric heat.

It therefore became important to examine on an experimental basis the theory that reversible heat effects are associated with cells giving anomalous electromotive forces.

Method.

The method usually adopted may be briefly described as follows :—The experimental cell was constructed of a thin

* Trans. Connect. Acad. vol. iii. pts. 1 and 2.

glass beaker as the outer vessel, containing one of the metal electrodes in a solution of one of its salts, and a porous earthenware cell, which contained the other metal in a solution of its corresponding salt. Cells of the Raoult form could not be employed on account of their inconvenience for thermal measurements and their necessarily high internal resistance. Attempts to reduce resistance by substituting animal membrane, parchment-paper &c. for the porous cell were abandoned owing to the facility with which they allowed small but significant quantities of the solutions to mingle by diffusion, and so permit the deposition of one metal on the more electropositive one, generating local heat and altering the electrical constants. The cell was supported by the rim of the beaker resting on a cork edge which was fitted into the top of a tin cylinder, the latter being itself suspended within a larger cylinder of bright tin. With this apparatus, when containing the usual working quantities of solutions, the rate of loss of heat was equal to $\frac{1}{100}$ of a gram-degree Centigrade for each minute-degree above the surrounding air temperature, for the few degrees of the experimental range.

Thermometers graduated to fractions of a degree were inserted, one in the inner porous cell and another in the outer cell; and in cases in which a slight inequality of heating at the two electrodes occurred, a mean of the rise of temperature of the two thermometers was taken as best expressing the net heat-evolution.

The external battery, used for sending currents against the E.M.F. of the experimental cell, consisted of either a Daniell cell or a form of constant bichromate battery, in accordance with the E.M.F. of the experimental cell to be overcome. The current was measured by a calibrated galvanometer, which gave a deflexion of $47^{\circ}5$ with $\frac{1}{10}$ of an ampere, and was regulated by the introduction of the necessary resistance.

It was first proved in the case of a cell, cadmium-copper sulphate, which gives an E.M.F. almost exactly in accordance with its calculated value, that the heat which is generated by a current passing through the cell depends only upon its resistance, and is independent of the direction in which that current passes.

This being so, if the permanent experimental value of the E.M.F. of any cell differs by e volts from its calculated value ($E - E_a = \mp e$), an amount of heat equal to $\frac{ect}{J}$ ought to be evolved or absorbed when a current of strength c passes for t seconds through the cell.

Thus if the cell be one giving an excess, or $E - E_h = +e$, there should be an absorption of heat when the cell is worked forwards and an evolution when worked backwards against the E.M.F. of the cell. Exactly the reverse would hold with cells giving a deficit from the theoretical value.

Since the heat due to resistance will be evolved in both cases, we have for the net heat evolved

$$H = \frac{c^2 r t \mp e c t}{J};$$

and where the value of $e c t$ is negative we have a negative total if it exceed the value of $c^2 r t$.

In order therefore that the *absorption* of heat may be a maximum, with cells giving an excess of E.M.F. when worked forwards, or with cells giving a deficiency when worked backwards, the current-strength must be adjusted in accordance with the determined value of the internal resistance of the particular cell.

Since the heat absorbed will be

$$-H_1 = \frac{e c t - c^2 r t}{J},$$

or

$$-H_1 \propto e c - c^2 r,$$

and

$$\frac{dp}{dc} = e - 2c r;$$

equating to maximum value we have

$$c = \frac{e}{2r}$$

as the current-strength giving the greatest absorption of heat in the case of a cell in which the computed and observed E.M.F.s differ by e volts and internal resistance is r ohms.

It is thus a matter of importance to determine the internal resistance, not only in order to be able to calculate the heat generated against it, but also to calculate the best current-strength for observing the effects. In many cells, however, the resistance is so unavoidably high that c would be too small for any thermal changes to be detected; and in other cases the current has to be increased above the best theoretical to a best practical strength, *time* being an important factor in such delicate thermal measurement.

It was found experimentally that the resistance in many cells is subject to considerable and, in some cases, enormous variations when currents are passing through the cell, being partly dependent upon the direction of the current. This

forms one of the most serious difficulties in the experimental work, for if the resistance change with change of direction of the current, the difference of heating-effects might be due, at least in certain cases, to such change rather than to the value of $E - E_h$.

This difficulty was endeavoured to be met by using a high resistance galvanometer on a circuit of several thousand ohms as a measurer of the difference of potential between the poles of the experimental cell, when on open circuit and when direct or inverse currents are passing through it.

Taking E as the E.M.F. of the external battery and R its resistance, and E_i the E.M.F. of the experimental cell, and r_i its resistance, and letting r_{II} denote the total external resistance, including that of the current-galvanometer, we get for the difference of potentials between the poles of the experimental cell :

$$V = \frac{E_i(R + r_{II}) - E r_i}{R + r_i + r_{II}} \text{ with direct currents,}$$

and

$$V_i = \frac{E_i(R + r_{II}) + E r_i}{R + r_i + r_{II}} \text{ with inverse currents.}$$

Since, however, the value of E_i (or indeed E) is subject to possible variation due to polarization, the above method fails to distinguish between change of difference of potential due to change of resistance and that due to polarization; and although the latter, if present, may be usually detected by suddenly interrupting the current and noting the value of E_i , the difficulty still remains to some extent and is liable to vitiate the results.

From the causes just indicated it will be readily seen that extremely few cells are suited for thermal experiments when currents of moderate strength have to be sent through them in alternate directions. Consequently a large number of voltaic combinations were tried and abandoned as unsatisfactory, for some of the following reasons:—Certain cells are found to give marked variation in their internal resistance with different direction of current, commonly owing to the sparing solubility of at least one of the salts formed and the consequent coating of one or both plates with insoluble and badly conducting films (*ex. gr.* cells involving the sulphate, chloride, or bromide of lead, mercury, or silver, three typically exceptional metals). In other cases cells are rendered useless by the imperfect reversibility of the chemical change; as in cases in which it is attempted to deposit a metal which can replace hydrogen from water at all readily (*ex. gr.* mag-

nesium, aluminium, zinc, and iron), hydrogen is liable, in some cases certain, to be liberated in place of the equivalent of metal. In other cells, such as lead or silver in contact with their sulphates or free sulphuric acid, on making either of those metals the anode in the cell, a peroxide (PbO_2 or AgO) is apt to form in spots on the surface of the metal plate and entirely alter both the E.M.F. and resistance of the cell, besides obviously altering the chemical work of electrolysis.

The desiderata in a cell intended for this work—namely tolerable freedom from polarization, perfect reversibility, low and constant internal resistance—are only even approximately fulfilled by a very few of a large number of cells examined by me, and therefore the experimental results are greatly restricted.

Mercury.

After dismissing aluminium and magnesium as being unsuitable, for reasons already stated, the metal which has the next highest “thermovoltaic constant” positive in sign is mercury, and being the metal which is replaced by most other metals, as copper, zinc, &c., it yields electromotive forces considerably in excess of the values deduced from the heat of formation of its salts. The insolubility and sparing solubility of the mercurous chloride, bromide, and sulphate, and the inadmissibility of mercuric salts, owing to their reduction in contact with metallic mercury, rendered it necessary to employ mercurous nitrate, and, accordingly, a copper-mercury nitrate cell was prepared, in which each metal was in a strong solution of its own nitrate, with a small proportion of free nitric acid, which is necessary to keep the mercurous salt in solution by preventing the formation of basic salts, and also diminishes the internal resistance. I found this cell gave an E.M.F. of $\cdot 43$ volt, while from the thermochemical numbers of Julius Thomsen, viz.

$[\text{Cu}, \text{O}, \text{N}_2\text{O}_5 \text{ Aq}] = 52410$ and $[\text{Hg}_2, \text{O}, \text{N}_2\text{O}_5 \text{ Aq}] = 47990$, one would deduce an electromotive force of only $\cdot 095$ volt.

(Wright and Thompson, adopting the older standards of measurement, give $\cdot 433$ volt for the experimental and $\cdot 097$ volt for the calculated value.)

When a current of $\cdot 2$ ampere was passed in alternate directions through the cell, it was found that if the current was not continued for too long a period in one direction the resistance was moderately constant, though by the prolonged action of the current the solution became either nearly deprived of mercury, or saturated with mercurous nitrate in

the layers near the metallic mercury, depositing crystalline nitrate on it and greatly increasing the resistance. But by allowing the current to pass for not more than 10 to 20 minutes in one direction this was avoided, and the rise of temperature which was observed corresponded very nearly with the heat due to $c^2 r t$, and was independent of the direction in which the current passed; showing therefore no absorption of heat with direct currents, as might be anticipated from its large excess of E.M.F., viz. .335 volt.

This result was so plainly at variance with what was to be expected, that a solution of mercurous nitrate was electrolysed, using a platinum anode; but in this case also there was no indication of more chemical work being performed by the current than supplied by it, *i. e.* no absorption of heat was detected, but an evolution corresponding closely with the amount calculated from the values of $\frac{c^2 r t}{J}$. Hence the re-

sult of these experiments clearly pointed to the conclusion that mercury, if it be anomalous in its E.M.F., is not so on account of any transformation of sensible heat into electric energy; and I was therefore led to examine the thermochemical data of mercury upon which the calculated E.M.F.s have been founded.

Thermochemical Data of Mercury.

In the first place Julius Thomsen* appears to be the only experimentalist who has made determinations of the heats of formation of mercury salts, and since he depends upon the heat of formation of mercurous nitrate for the thermal values of almost all the other salts, if that were in error all the values derived from it would evidently be equally inaccurate. But the method he employed for the determination of the heat of $[\text{Hg}_2, \text{O}, \text{N}_2\text{O}_5 \text{ Aq}]$ does not appear to me entirely free from objection. He determined it by reducing the mercury from a solution of its nitrate by sulphur dioxide, and determined the heat of neutralization of $[\text{Hg}_2\text{O}, 2\text{HNO}_3 \text{ Aq}]$ by decomposing the nitrate with sodic hydrate; finding, as above stated, 47990 calories for the heat of formation of the nitrate in aqueous solution. Since, however, mercurous nitrate requires the presence of free nitric acid to maintain it in solution, he used a solution containing free nitric acid to an extent variously stated as 1.079 and 3.079 HNO_3 per molecule of $\text{Hg}_2(\text{NO}_3)_2$; as the latter number is mentioned twice, probably the former is a misprint. But the exact

* *Thermochemische Untersuch.* Band iii. S. 355.

reaction which occurs between mercurous nitrate, free nitric acid, and SO_2 one may reasonably expect to be less simple than that required for exact thermal measurement.

I have accordingly determined the heat of formation of mercurous nitrate in acid solution, by decomposing it with zinc amalgam, yielding zinc nitrate and metallic mercury; the results obtained are subjoined.

Mercury used.	Heat-units cooled.	Heat of Reaction.
1.362 grms.	265.5 grms.° C.	77,975
1.362 "	206.2 "	78,180
2.58 "	492.1 "	76,300

Mean = 77,485

Since Thomsen finds 102,510 for $[\text{Zn}, \text{O}, \text{N}_2\text{O}_5 \text{ Aq}]$, the heat of formation of

$$[\text{Hg}_2, \text{O}, \text{N}_2\text{O}_5 \text{ Aq}] = 102,510 - 77,485 = 25,025 \text{ calories.}$$

If this be approximately true, it follows that the values given by Thomsen for the heats of formation of the chloride, bromide, and iodide of mercury must be greatly in excess of the true value. The heat of formation of $[\text{Hg}_2, \text{Cl}_2]$ is given by him as 82,550, taking 47,990 as the value for the nitrate. But it is well known that calomel is reduced to metallic mercury by an acid solution of stannous chloride, although the heat evolved in the conversion of $\text{SnCl}_2 \text{ Aq}$ into $\text{SnCl}_4 \text{ Aq}$ is only 76,030. Hence splitting up Hg_2Cl_2 with the formation of stannic from stannous chloride would give, according to Thomsen, the following thermal equation:—

$$\begin{array}{rcl} [\text{Sn Cl}_2, \text{Cl}_2, \text{Aq}] - [\text{Hg}_2, \text{Cl}_2] & & \\ 76,030 & - & 82,550 = -6520, \end{array}$$

so that this reaction would be markedly endothermic and could not well occur at ordinary temperatures.

The matter, however, was put to experimental test, and it was found that although the reaction was slow, requiring about three hours for its completion at a temperature of about 15°C ., still heat was continuously *evolved*, and, after calculating for loss of heat, gave the following values:—

Weight of Hg_2Cl_2 .	Heat evolved.	Heat of Reaction.
5.067 grms.	120.76 grms.° C.	11,225
4.783 "	104.8 "	10,325

Mean = 10,775

Giving $76,030 - 10,775 = 65,255$ calories as the heat of formation of Hg_2Cl_2 .

The electromotive forces calculated from these thermal

data give for zinc-mercury nitrate cells $E_h = 1.673$ volt, the experimental value being 1.5 volt; and for zinc-mercury chloride cells $E_h = 1.028$, the experimental value being 1.043 (Helmholtz), or from 1.123 to .988 (Wright and Thompson). Hence it is evident that mercury cells do not evolve electromotive forces of about half a volt in excess of their calculated values, the various statements to that effect being based on inaccurate thermochemical data. On the contrary, it will be noticed that the value of E_h for the nitrate cell is greater than the experimental value E , which may be due to an error in my determination of the heat of formation of mercurous nitrate, but more probably to the same cause acting as in the cases of lead and silver, both of which metals have negative "thermovoltaic constants," and which greatly resemble mercury in many chemical characteristics.

Silver.

The "thermovoltaic constants" for silver, given by Wright and Thompson, vary from .395, in the case of the nitrate, to .02, in the case of the iodide, but are all negative in sign. It is noticeable that the larger values are attached to the soluble salts, while the insoluble chloride, bromide, and iodide have much smaller "constants," and these depend largely on the nature of the liquid in which the haloid salt is suspended; thus AgCl in solution of ZnCl_2 has a thermovoltaic constant of .112 to .062 volt, while a copper-silver chloride cell in which the AgCl is suspended in CuCl_2 solution has a difference of only .001 volt from the computed value. The causes producing the large divergence in the case of the nitrate or sulphate are, therefore, probably different in nature from those producing the small and very variable "constants" (?) in the case of the chloride, bromide, or iodide.

In order to test whether a cell consisting of silver in a solution of its nitrate, opposed to some other metal, gives rise to reversible heat-effects or not, a copper-silver nitrate cell was prepared and used in the manner previously described. It was found that when small currents (.1 to .2 amp.) were sent in alternate directions through the cell by means of another battery, apart from the heat due to the square of the current and the resistance, there was an evolution of heat when the cell was worked forwards, and an absorption of heat when worked backwards so as to dissolve silver and deposit copper.

The following numbers, taken from one set of experiments, show the difference of heating with synergetic and opposed currents (E.M.F. of the cell .431 to .428 volt):—

Mean rise of Temp.

Direct current of .2 ampere for 20 minutes	. .	0.6 C.
Adverse current	”	0.1 C.
Direct current	”	0.35 C.
Adverse current	”	0.00 C.
Direct current	”	0.45 C.
Adverse current	”	0.00 C.

Mean rise with direct currents = $0^{\circ} \cdot 466$ C.Mean rise with adverse currents = $0^{\circ} \cdot 03$.

(Commonly = 0).

The thermal capacity of the cell was determined and found to be equal to 68 grms. of water ; so that

$$68 \times .466 = 31.688 \text{ grms.}^{\circ} \text{C.}$$

represents the total mean heat evolved when the cell was worked forwards, and due to

$$\frac{c^2 r t + e c}{J}.$$

The mean resistance of the cell was found to be 1.4 ohm, and taking e , the difference between E and E_h , as .34 volt, we get

$$\frac{c^2 r t}{J} = .04 \times 1.4 \times 1200'' \times .24 = 16.1 \text{ grms.}^{\circ} \text{C.}$$

and

$$\frac{e c t}{J} = .34 \times .2 \times 1200'' \times .24 = 19.58 \text{ grms.}^{\circ} \text{C.}$$

Hence the sum of these amounts, 35.68 grms. $^{\circ}$ C., and their difference, -3.48 grms. $^{\circ}$ C., represent the total amounts of heat evolved with direct and with adverse currents respectively, with which the experimental results of 31.69 , and practically 0, accord within the limits of experimental error.

Hence it results that a copper-silver nitrate cell gives an electromotive force below the calculated value, and when it sends a current a portion of the energy due to the replacement of silver by copper appears as heat in the cell, and its equivalent in E.M.F. is therefore wanting. Consequently, when the cell is worked backwards, a smaller amount of work than that represented by the negative heat of substituting silver for copper is capable of producing the required electrolysis, the difference being supplied at the expense of sensible heat.

In view of the great difference in the heat of formation of mercury salts found by me, and the previously recorded

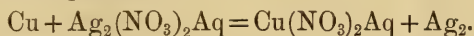
values, I determined the heat of replacement of silver by copper from a solution of AgNO_3 , $200\text{H}_2\text{O}$. The amount of silver was accurately determined both by titration of the solution and by calculation from the weight of the dissolved copper.

The results of two exactly similar experiments gave

$$\text{I.} = 33,420 \text{ calories,}$$

$$\text{II.} = 33,450 \text{ calories,}$$

as the heat of replacement :—



These numbers are slightly below those found by Thomsen, he giving 35,630 as a mean ; but they are sufficiently in agreement to show that the actual E.M.F. furnished by a copper-silver nitrate cell is considerably less than the calculated value.

It has been above remarked that the “thermovoltaic constants” of silver vary greatly with the salt of the metal employed, the soluble salts having much larger constants than the insoluble, and the latter diminishing to an insignificant quantity in the case of the iodide, or of the chloride suspended in cupric chloride. This would therefore show that it probably does not depend in any way upon the metal itself acting as a thermoelectric junction, but upon the nature of the chemical changes occurring in the cell.

From these, and other considerations which are given later, I conclude that the thermovoltaic constant depends upon two prime causes. One, which operates with both soluble and insoluble salts of silver, is a tendency of that metal to form sub-salts, *e. g.* subchloride Ag_2Cl , the heat of formation of the equivalent of which is greater than that of the normal salt, so that silver is more readily attacked superficially and acquires a lower relative potential than would be predicted from the heat of formation of its normal salt.

Wetzlar has shown that silver-leaf immersed in solutions of cupric or ferric chloride becomes converted into Ag_2Cl , so that when a silver plate is immersed in cupric-chloride solution its surface rapidly becomes covered with a film of the subchloride of sufficient thickness to prevent the metallic silver producing its usual effect, and thus the thermovoltaic constant practically vanishes.

The other cause of divergence is of much wider application, and appears to me to depend upon the fact that with any salt, such as silver nitrate, which does not form a hydrate with water, but being soluble absorbs heat on dissolving, this heat can be supplied by the thermometric heat of sur-

rounding bodies, and one would expect it to be "bound energy," and to be a reversible thermal effect independent of the effects due to the "free energy" of the chemical change.

Since the heat of solution of silver nitrate is a large negative quantity, -5440 or $-10,880$ for $\text{Ag}_2(\text{NO}_3)_2, \text{Aq}$, the heat of formation of anhydrous AgNO_3 would be greater by that amount than that measured when using its solutions. This would give the heat of formation of

$$[\text{Ag}_2, \text{O}, \text{N}_2\text{O}_5] = 16,770 + 10,880 = 27,650,$$

and thus give a calculated E.M.F. about $\cdot 235$ volt lower, still leaving a deficiency of about $\cdot 105$ volt to be accounted for by the tendency of silver to form a film of subsalt; unless indeed the heat of formation of silver nitrate has been under-estimated, as the numbers given above for the heat of replacement of silver by copper might indicate.

These numbers, after allowance for the negative heat of solution of silver nitrate, would give a calculated electromotive force almost exactly in agreement with the experimental value.

Lead.

Lead, like silver, is a metal having a large negative "thermovoltaic constant," which also varies with the salt of the metal employed. Wright and Thompson give for the difference of E and E_h in the case of lead in saturated solutions of its chloride and bromide $\cdot 222$ and $\cdot 235$ volt, while for the iodide only $\cdot 002$ volt. This difference, however, is readily explained on reference to the numbers selected by these authors in calculating the theoretical E.M.F., as, in the case of the chloride and bromide, they employ the heats of formation of the dissolved salts, while they use the heat of formation of solid lead iodide. Inasmuch as they used *saturated* solutions of the chloride and bromide, it is difficult to see why they selected the values corresponding to the dissolved salts, since any more salt formed must necessarily be produced in the solid state, and the electromotive force would, consequently, be determined by the heat of formation of the solid salts.

Further, since there is no chemical attraction between these compounds and water, and there is merely an absorption of heat on solution, in accordance with the view stated above (in regard to silver nitrate), the E.M.F. will depend upon the heat of formation of the solid salt, even though the solution be not saturated.

The values calculated from the dissolved and anhydrous salts and the experimental values are compared below:—

	Observed Average Value.
$[\text{Zn}, \text{Cl}_2, \text{Aq}] - [\text{Pb}, \text{Cl}_2, \text{Aq}] = 36870 \text{ c.} = \cdot 796 \text{ volt}$	} Volt. ·591
$[\text{Zn}, \text{Cl}_2, \text{Aq}] - [\text{Pb}, \text{Cl}_2] = 30070 \text{ c.} = \cdot 649 \text{ volt}$	
$[\text{Zn}, \text{Br}_2, \text{Aq}] - [\text{Pb}, \text{Br}_2, \text{Aq}] = 36550 \text{ c.} = \cdot 789 \text{ volt}$	} ·571
$[\text{Zn}, \text{Br}_2, \text{Aq}] - [\text{Pb}, \text{Br}_2] = 26510 \text{ c.} = \cdot 572 \text{ volt}$	

The zinc-lead iodide cell being calculated from the heat of formation of solid Pb I_2 accords almost exactly with the experimental value, the difference being only ·002 volt.

Similarly with a zinc-lead nitrate cell, while the E.M.F. calculated from the heat of dissolved lead nitrate is ·737 volt, after allowing for the large negative heat of solution, -7600 , one gets for $[\text{Zn}, \text{O}, \text{N}_2\text{O}_5, \text{Aq}] - [\text{Pb}, \text{O}, \text{N}_2\text{O}_5]$, $102,510 - (68,070 + 7600) = 26,840$, giving a calculated E.M.F. of ·580 volt, which agrees exactly with the experimental value ·580 to ·591 volt.

A reference to the effect of concentration of the solutions on the electromotive force of a zinc-lead nitrate cell confirms my views. Wright and Thompson find that increasing the solution strength from ·25 to 2 $\text{M}''(\text{NO}_3)_2$, 100 H_2O causes an increase in the E.M.F. from ·580 to ·591, while they calculate that it should decrease from ·759 to ·716. Hence, instead of a *fall* of ·043 volt there is a *rise* of ·011 volt, the cause of the increase being in all probability due to the fact that the heat of dilution of zinc nitrate is positive in strong but negative in weak solutions; so that with moderately strong solutions the whole of the combining energy of zinc nitrate with water may be converted into electric energy, while in weak solutions a portion of that part of the energy of chemical change is expended in the absorption of heat due to the greater dilution of the salt.

It may be well to give here some collateral evidence in support of this view. In the first place, if lead in chloride solutions, either not saturated with or entirely free from *lead* chloride, had a tendency to form Pb Cl_2 , represented by the heat of formation of the dissolved salt, viz. 75,970 c., this metal should be without action on a solution of stannous chloride, the heat of formation of which in aqueous solution is 81,140 calories; but, as a matter of fact, a piece of pure clean lead placed in an acid solution of stannous chloride becomes immediately covered with a dull grey film, and after a short time small brilliant crystals of deposited tin appear all over its surface. For the same reason a cell consisting of lead and tin plates, both immersed in either dilute hydrochloric acid, stannous chloride, or lead chloride solutions, furnishes a current the direction of which indicates that lead and not tin

is the metal attacked, the actual E.M.F. being about .008 to .012 volt. On the other hand, pure tin will not remove lead from solutions of its nitrate or acetate or cold solutions of its chloride, though if an acid solution of lead chloride be boiled with tin, lead is deposited to a slight extent. Similarly the above lead-tin cell, using an acid solution of stannous chloride, will be found to diminish in E.M.F. as its temperature is raised, so that between 50° and 60° C. it possesses no E.M.F., while above that temperature the current flows in the reversed direction, tin being the metal attacked. This explains why an alloy of lead and tin, when treated with hot hydrochloric acid, yields a solution of stannous chloride, leaving the lead unattacked. That the non-production of lead chloride cannot be due to the prospective absorption of heat on dissolving *after its formation* appears sufficiently evident; and I think this reversed action at the higher temperature is to be explained by the heat of formation of the compound $\text{SnCl}_2 \cdot 2\text{H}_2\text{O}$ being greater than that of even solid lead chloride (viz. 86,510 and 82,550 respectively); and although the former would be dissolved with a certain absorption of heat dependent on the strength of solution and the temperature, this amount would be less than the 5370 calories absorbed at low temperatures and in dilute solutions, and, further, being due to the merely physical process of solution, may at the high temperature be supplied largely at the expense of sensible heat, instead of being deducted from the energy of the chemical change.

Nickel is a metal in the formation of the anhydrous chloride of which 74,530 calories are evolved, while in aqueous solution it furnishes 93,700 calories. If, therefore, the heat of formation of the dry salt plus the heat of hydration be regarded as measuring the total chemical energy and as occurring together, nickel should be able to replace tin or lead from their chloride solutions. But, as a matter of fact, instead of this being the case, pure electro-deposited tin is capable of completely displacing nickel from a solution of its chloride containing free hydrochloric acid, when allowed to remain in contact for a considerable time; and on boiling the mixture of tin and deposited nickel in hydrochloric acid, tin and not nickel is dissolved. Nitric acid, however, rapidly attacks the nickel.

This result is at once explained on reference to the heat of formation of anhydrous nickel chloride, which, as above stated, is given by Thomsen as 74,530, while the heat of formation of stannous chloride is 81,140 in aqueous solution, or 80,790 if anhydrous. The heat which would on the whole be absorbed by this replacement in aqueous solution would doubtless be supplied by surrounding temperature.

These considerations, therefore, show that the heat of formation of the anhydrous salt is the most important factor in determining the chemical reaction, and suggest that the kind of energy set free in the formation of a dry salt differs from that due to the hydration of that salt, the latter being only partly "free energy," while the negative heat of simple solution may be supplied entirely by the "bound energy" of the chemical change.

A similar explanation will account for the fact that an iron-cadmium chloride cell gives an E.M.F. opposite in direction to that anticipated from the heats of formation of their salts in solution, since Thomsen gives $[\text{Cd}, \text{Cl}_2] = 93,240$, $[\text{Cd}, \text{Cl}_2, \text{Aq}] = 96,250$, and $[\text{Fe}, \text{Cl}_2] = 82,050$, $[\text{Fe}, \text{Cl}_2, \text{Aq}] = 99,950$.

Thus, taking the anhydrous salts we get a difference of 11,190 on the side of cadmium, while taking the dissolved salts gives 3700 calories in favour of iron being attacked and depositing cadmium, which is known not to occur. Thus the first action in point of time—the formation of the anhydrous salt—directs the course of the chemical change, which, however, in the present case is not a replacement of iron by cadmium, but if a current flow, hydrogen is evolved on the iron plate; so that this cell does not reverse the expected reaction and furnish a current at the expense of sensible heat, but, owing to the initial difference in the heats of formation of the dry salts, an entirely different reaction is started and continued.

It is owing to the fact that iron is not deposited electrolytically from neutral or acid solutions of its salts that its large positive constant was referred earlier in this paper to a false reaction, though the above consideration shows at least one cause for its anomalous behaviour.

Tin.

Measurements of the electromotive force of cells in which tin in a solution of its chloride is opposed to other metals having shown* that it possesses an apparent negative "thermovoltaic constant," it was resolved to employ a cell in which tin is the metal attacked, and which should consequently give an E.M.F. in excess of the heat evolution due to the chemical change. Copper, which naturally suggests itself as the opposing metal, is, however, very unsatisfactory for thermal measurements when used in a solution of its chloride, owing

* Phil. Mag. ser. 5, vol. xxi. p. 13.

to the production of cuprous chloride on its surface, and further, as Thomsen found for the heat of replacement of tin by zinc from solution of its chloride numbers varying from 28,940 to 33,510 calories (a variation of 4570 calories, or .098 volt), a determination of the previously unknown heat of formation of stannous sulphate was made with the object of using a tin-copper sulphate cell.

A solution of copper sulphate, each cubic centimetre of which contained .015875 grm. of copper, corresponding to $\cdot 45\text{Cu SO}_4 \cdot 100\text{H}_2\text{O}$, and containing free sulphuric acid to the extent of 2 per cent., was treated in the calorimeter with excess of pure granulated tin; the copper was speedily deposited, and the following heat measurements were made:—

Weight of Copper.	Heat evolved.	Heat of Reaction.
$\cdot 635$ grm.	190.36 grm. $^{\circ}$ C.	19,036 calories.
$\cdot 635$ "	193.14 "	19,314 "
$\cdot 635$ "	193.98 "	19,398 "
Mean . .		19,250 "

As Thomsen finds for the heat of formation of copper sulphate 55,960, this number added to 19,250 gives the heat of formation of $[\text{Sn}, \text{O}, \text{SO}_3 \text{Aq}] = 75,210$ c.

In the above reaction there was no evolution of hydrogen gas, even after the whole of the copper had been deposited, and the conditions were as similar as possible to those obtaining in the case of the same replacement in the voltaic cell. But in the next experiment, which was conducted partly as a control on the previous values, a small amount of hydrogen was produced towards the end of the reaction. The solution of stannous sulphate obtained in the previous reaction was decomposed by placing in it strips of pure cadmium, and the heat of the reaction $\text{Sn SO}_4\text{Aq} + \text{Cd} = \text{CdSO}_4\text{Aq} + \text{Sn}$ was accordingly measured, giving 15,210 c. as the result. Since $[\text{Cd}, \text{O}, \text{SO}_3 \text{Aq}] = 89,880$, the difference $89,880 - 15,210 = 74,670$ represents the heat of formation of $[\text{Sn}, \text{O}, \text{SO}_3 \text{Aq}]$, as measured in this way, and agrees fairly well with the result obtained by the replacement of copper by tin, but is slightly lower, probably owing to the above-mentioned slight evolution of hydrogen, and therefore excess of the heat of replacement of tin by cadmium.

As a further check upon the general method I determined the heat of replacement of copper by zinc, using the same solution, viz. $\cdot 45\text{CuSO}_4 \cdot 100\text{H}_2\text{O}$, but, of course, free from sulphuric acid, and found the heat of reaction to be 50,980 c., the number calculated from Julius Thomsen's values being

50,130 for solutions containing $\cdot 25\text{CuSO}_4 \cdot 100\text{H}_2\text{O}$, which numbers I considered to be in sufficiently close agreement.

But adopting 19,250 c. as the heat of replacement of copper by tin we get a calculated E.M.F. of only $\cdot 416$ volt, while experiment shows an E.M.F. of $\cdot 55$ volt as a mean value; thus showing that a tin-copper sulphate cell furnishes an E.M.F. of about $\cdot 144$ volt in excess of its computed value. Using this cell in a calorimeter, I found that with direct currents of $\cdot 1125$ ampere passing through the cell there was an absorption of heat, and when worked backwards against the E.M.F. of the cell there was an evolution of heat apart from that due to the square of the current and the resistance. The internal resistance in one of these cells was reduced to only $\cdot 9$ ohm, so that the heat due to it was very small.

I conclude from these observations, and in consonance with the theory already stated, that stannous sulphate has a negative heat of dilution, which portion of its total energy, while appearing as negative in the calorimeter, is unable to reduce the electromotive force due to the "free energy" of the undiluted salt, the negative heat of dilution being supplied by fall of temperature. I have not been able to verify this conclusion experimentally, and the subject is complicated by the necessity of having free acid present in its solutions to prevent the formation of basic salts. The effect both of dilution and of the quantity of free acid present also greatly influence the E.M.F. of stannous chloride cells, and some points in regard to its action may be seen from the following consideration.

It is well known that if a strip of tin be placed vertically in a vessel, containing at its lower part a strong solution of stannous chloride (with free hydrochloric acid) and above it water, at the line of junction of the liquids, where a dilute solution of stannous chloride is formed by the intermixture, metallic tin is deposited in brilliant crystals on the tin plate, tin dissolving in the liquid below. This solution of a metal in a concentrated solution of one of its salts and its deposition from a dilute one is entirely contrary to what would be expected, were it not for the fact that in the relatively strong hydrochloric acid in the lower liquid there is a store of potential chemical energy which may either evolve heat in diffusion and dilution, or, by attacking the tin plate in contact with it, furnish a current which, flowing through the liquid and metal, has the effect of depositing an equivalent amount of tin from the solution above, in which the hydrochloric acid, being more dilute, has less tendency to dissolve the tin, a considerable portion of its energy having already been expended in the heat evolved during dilution.

Hence in tin cells, while I recognize a negative "thermo-voltaic constant" which is capable of producing reversible thermal effects in the cell, I am inclined to refer them to two causes: (α) a negative heat of dilution of the salts, and (β) to effects of differences of concentration of the acid necessarily employed to keep the salt in solution*.

General Conclusions.

A review of the published measurements of the thermal values of chemical reactions, in particular those directly relating to electromotive forces, namely, the replacement of one metal by another from its dissolved salt, will, I think, show that however valuable they may be as guides they cannot be implicitly relied upon as furnishing absolute and unimpeachable data by which to judge the truth or fallacy of a given theoretical conclusion. In the first place, in the numbers representing the heat of formation of the same body, determined by different experimenters, there are frequently discrepancies of considerable magnitude. Thus Thomsen finds for the heat of reaction of lead acetate and zinc 34,950 cal., Favre finding only 31,200 c., while Andrews gave 37,710 c.; a total variation between the extremes of 6,510 calories. For the heat of formation of CuO, and therefore of all salts derived from it, Thomsen gives 37,160, Favre and Silbermann 43,770, and Andrews 38,300, while $[\text{Fe}, \text{Cl}_2, \text{Aq}]$ is variously estimated by Thomsen at 99,950, by Favre and Silbermann at 106,700, and by Andrews at 102,060.

Besides these experimental differences there is always the fact that, as Berthelot has clearly laid down in his introduction to his *Essai de mécanique chimique*, the quantity of heat evolved in a reaction measures *the sum of the physical and chemical changes* which occur in the reaction; some of which quantities may be positive or negative, and which may or may not directly influence the transformable energy of the chemical change.

Adopting the view of Helmholtz that a chemical process gives rise to a total amount of energy, part of which is freely transformable and part of which is "bound energy," while the calorimeter measures the total energy, the electromotive force of a voltaic cell is a measure of the actual transformation of free energy. In cases, therefore, in which a portion of the total energy is to be reckoned as negative

* In my previously published measurements of the E.M.F. of tin cells the value given for the cadmium-tin sulphate cell was only .189 volt; but I have since found that, using more dilute and less acid solutions, a persistent value as high as .234 volt may be obtained.

in respect to the main reaction, we see that such portion may be supplied either at the expense of the free or of the bound energy. If performed by the free energy, obviously the amount of that energy which could appear in other forms must be proportionately reduced; while if performed at the expense of the bound energy, merely thermal effects would follow, without affecting the course or amount of the free-energy stream. It follows that in certain cases, as for instance in the case of lead nitrate, or silver nitrate, if the heat of solution is a large negative quantity, the free energy, in my opinion, is greater than the total energy, since the bound energy is negative in sign; but, assuming that this negative bound energy is not supplied at the expense of the free energy, it can be readily made up by thermometric heat. Hence, while thermal measurements indicate $27,600 - 10,880 = 16,720$ as the heat of formation of silver nitrate in solution, the voltaic cell shows that at least 27,600 thermal units must be regarded as transformable free energy, the 10,880 negative heat-units being abstracted not from it, but from sensible heat. On the whole there must be an increase of entropy, but there may be an absorption of heat, as when a lead-copper nitrate cell sends a current, or a copper-silver cell is worked backwards.

The partial independence of thermal effects due to concentration of solutions and electromotive forces, is recognized by Helmholtz when he states in the case of a Latimer Clark cell with saturated zinc-sulphate solution, that "the zinc sulphate, newly formed by the current, can no longer be dissolved, and its latent heat of solution is saved; accordingly there is *a more powerful development of heat in the cell in spite of the weaker electromotive force.*" His theoretical mathematical treatment disposes of many difficulties, but it did not prevent him from the error of believing that mercury, when deposited by zinc, even from its solid salts, gave rise to an E.M.F. largely in excess of the thermal values, which, according to my view, would be quite impossible, as it would amount to a conversion not merely of "bound energy" into "free energy," but, under special circumstances of the thermometric heat, into higher forms of energy with the production of mechanical effect.

It might be thought that the whole of the heat liberated in the formation of the fully hydrated salt was a measure of the available energy, and that the negative heats of dilution were supplied solely at the expense of sensible heat, but this supposition is entirely negated by the actual electromotive forces of several cells (*ex. gr.* zinc-cadmium bromide cell); so

that I conclude that since the hydration and solution of a newly formed salt will, to a great extent, occur simultaneously, the heat of hydration is partly absorbed in the process of solution and dilution, and therefore to that extent cannot furnish electrical energy. On the other hand, when a salt is used which does not form hydrates, and consequently can *only* absorb heat during solution, the energy due to the formation of the solid salt determines the E.M.F., which cannot be subsequently decreased by a process which has no chemical continuity with it, but which merely absorbs sensible heat.

As Helmholtz has already remarked, the exact reversibility of these processes indicates the constancy of the partition of "free" and "bound," or electrogenic and thermogenic energy, and negatives the earlier supposition of Braun that the proportion of transformable energy was the accident of circumstances.

Helmholtz has further shown that, regarding the energy and entropy of a system undergoing a restricted change as represented by the differential coefficients of an integral function of the system or substance, under the usual conditions the performance of external work takes place at the expense of the "*free energy*," while loss of heat at the expense of the "*bound energy*." But whether there subsists a definite relation between the freely transformable and the bound energy on the one hand, and on the other hand between that portion of the energy set free by the process of hydration of a salt which is convertible into electric energy, and that portion which runs down into heat, is a question which appears to demand careful experimental as well as mathematical consideration; especially with reference to the influence of temperature and various degrees of concentration of the salt.

The fact that the E.M.F. of a cell, like zinc-cadmium chloride, does not increase with greater dilution as rapidly as it should from thermochemical considerations, may be taken as indicating that the degradation of energy of the system increases as the number of water-molecules in which the salt is dissolved is augmented.

I conclude, therefore, that :—

I. The primary factor in determining the electromotive force of a voltaic cell is the relative heat of formation of the anhydrous salts of the two metals employed.

II. That the E.M.F. may set up chemical changes of a different direction and character from those predicable from the heat of formation of the dissolved salts.

III. That the E.M.F. set up by (I.) may be, and usually

is, supplemented by the energy, or a portion of the energy, due to the hydration or solution of the solid salts, and may have values which accord with the heat of formation of the dissolved salts.

IV. That in those cases in which there is no chemical attraction, or a very feeble attraction between the water and the salt, the negative heat of solution is derived from sensible heat, and is not supplied by the free energy of the chemical change. All cells in which such salts are employed opposed to zinc should have *negative* "thermovoltaic constants," and evolve heat when they send a current forwards.

V. That when metals, whose salts have purely negative heats of solution, are opposed to metals whose salts they can replace, the E.M.F. set up is *in excess* of the total thermal change. Such cells, therefore, absorb sensible heat when worked forwards.

VI. That, taking the foregoing facts into consideration, no cell exists which can furnish an E.M.F. in excess of the free energy of the chemical change; *i.e.* which can convert sensible heat into electric energy working at uniform temperature. (Negatives the supposition concerning mercury and other cells.)

VII. That certain metals have a tendency to form films of sub-salts on their surfaces, the formation of which giving rise, as it does, to a different thermochemical reaction, naturally furnishes an E.M.F. which does not correspond with the values calculated from the heats of formation of their normal salts. (*Ex.gr.* copper in cupric chloride, mercury in mercuric chloride, probably silver in most soluble chlorides.)

VIII. That the electromotive force of a voltaic cell furnishes a more accurate measurement of the "free energy," and therefore of true chemical affinity, than data derived from calorimetric observations.

XXVI. *A Consideration of the Effects of Contraction during the Cooling of Intrusive Masses of Granite and the Cause of their Solid Continuity.* By T. MELLARD READE, C.E., F.G.S., F.R.I.B.A.*

NO doubt many geological observers have wondered, on looking at an intrusive granite-mass such as Cairnsmore of Fleet in Kirkcudbrightshire, or that of Shap Fells in

* Communicated by the Author.

Westmoreland, to find so little evidence of contraction. If, as we have every reason to believe, the granite has once been in a state of igneous fluidity, it would at first sight seem reasonable to expect more evidences of contraction than what are usually manifest. The late David Forbes was so much struck with this in Norway that he instituted a set of experiments to test the statement of Bischof that granite contracts from 10 to 25 per cent. in volume in cooling. His results in the casting of highly siliceous slags showed only a contraction of from 0.014 to 0.028. Forbes did not attach great weight to these experiments as accurate determinations in consequence of the difficulty of getting a perfectly solid casting, but considered that they proved Bischof to have much overestimated the contraction of granite. Let us consider what the process of contraction is, and in what way granite-masses are likely to be affected by it.

In casting ironwork it is usual to allow in the pattern $\frac{1}{10}$ of an inch per linear foot for the contraction of the iron = $\frac{1}{120}$ of the whole length, or a contraction in volume of $\frac{1}{40}$. This is not, however, the contraction from a fluid state, for the exterior cools first, and, at the time of solidification, is as large as the pattern. The contraction of iron in passing from the fluid to the solid state or in "freezing" is therefore not determined by this practice, and I am not aware of any reliable determination of its amount; indeed some have contended that it expands in freezing, because cold iron will float in molten iron.

Be this as it may, I think we can get some hints of the operations of nature from the processes of the ironfounder. For instance, in casting a large sphere, say 2 feet in diameter, it is necessary, to prevent the honeycombing of the central part, to keep feeding the ball as the metal cools. It would seem that the outer skin takes the exact size and form of the mould on solidification, and the metal cools and congeals from the exterior to the centre. By keeping the centre fed with new molten metal a sound casting is insured. In the same way any one who has been in the habit of making castings in lead is aware of the sudden disappearance of the lead in the feeding-throat on congelation taking place, so that unless a good plug be left a deep hole will occur in the casting at the point where the metal has been poured in. Both these phenomena point towards sudden contraction on congelation.

Let us now consider the case of a granite-mass. It is extremely probable that in many cases these masses are more or less like what Mr. Gilbert has called Laccolites or stone

cisterns*, and are, in fact, reservoirs fed from a throat below. In other cases it may be possible that they are connected with the central igneous magma of the earth in a larger way, as tongues or protrusions of it. When this is the case it is difficult to realize the enormous amount of heat-energy brought up from below to be expended on the overlying sediments in the way of mountain-building. Whether the granite intrusion be fed by a throat, or whether it be a tongue of molten matter, as it cools and congeals within the bowels of the earth (which it must do at an extremely slow rate) the uprising stream will introduce compensating matter in the same manner as does the plug in casting. The only difference will be that the one runs in by gravitation, the other is forced up by pressure from below. But while the matter is rising it is also eating into the surrounding sediments and incorporating them into its own body. And it is well here to remember that granite is of a higher specific gravity than most sedimentary rocks, in the proportion of about 2.90 to 2.30, or, in other words, a ton of granite contains from 11.8 to 12.8 cubic feet, while a ton of sandstone contains from 14.3 to 17.3 cubic feet. Therefore, if we for the sake of illustration assume a sandstone to consist of the same elements as a granite, it will, on incorporation with the molten mass, actually decrease in volume. Of course the amount of decrease would vary with the composition of the sandstone. Again, if the surrounding rock were marl it would decrease in a greater ratio.

There are thus two forces at work—one the absorption of the surrounding rock and its decrease in volume, the other the introduction of new matter, the total effect tending towards disruption by increasing bulk. This effect may be seen in any good granite contact. There is generally a laminated gneissic band between the granite and the enclosing rock, and often they are so welded together that junction specimens showing both structures can be taken and cut for the microscope. It is not improbable that the fragments of gneissic rock found in granite may be, so to speak, broken off from the walls of the intrusion.

Sigas of lateral displacement of the surrounding rock to a considerable extent are also sometimes seen. But what is it that prevents the disruption and fissuring of the granite by contraction after solidification takes place?

I answer gravitation, for one thing; for so long as the rock will yield as a paste, the mass above, which in most cases is supposed to have been very considerable, will keep it com-

* Geology of the Henry Mountains.

pressed and in contact with the surrounding walls. When the granite has by cooling solidified sufficiently to be unaffected in this way, other agencies come into play. I have shown * that variations of temperature of a small amount extending over a large area will produce an intermittent creep of the strata in one direction, so that the surrounding rock is continually being forced up against the granite.

That considerable variations of temperature are constantly taking place in regions of intrusion is tolerably certain, and that the heat of these intrusive masses is very slowly imparted to the encircling rocks is shown by the extremely slow cooling of some lava-streams that are only protected from the atmosphere by a thin covering; whereas granite is often in enormous masses and deeply buried. Some of these masses must have taken thousands of years to cool. In addition to the lateral creep already spoken of, there is another force tending to compression in the gravitation of the surrounding and partially uplifted rock which naturally tends to close up any shrinkage of the intrusive mass.

It follows from this that granite-masses do not show signs of shrinkage such as Forbes looked for, not because the material does not shrink but because, from one reason or another, it is kept throughout its history in a state of compression. When the contraction of the under-mass takes place on a large scale, then the granite may be sheared by normal faulting, as I have described in chap. viii. 'Origin of Mountain-Ranges,' like any other rock-mass.

Granite intrusion may take place without creating much disturbance, but when it occurs in large masses, such as in the core of Mont Blanc and in other Alpine regions, from the amount of new matter forced in from below, it must become a very potent factor in producing lateral pressure. The fan-structure which we see in such regions is in my opinion produced by the gradual spreading out of the probably semi-plastic matter by gravitation; indeed it seems impossible that it can be due to any other cause. This idea is not a new one by any means; the celebrated architect Viollet le Duc has shown that expansion and intrusion of the protogene in the core of Mont Blanc has caused the folding back of the beds which it has forced apart †. The effects of gravitation as a geological agent has lately been

* Origin of Mountain-Ranges, p. 329.

† Mont Blanc, by Eugène Viollet le Duc; translated by B. Bucknall, 1877, chap. i.

treated of by Mr. W. Barlow in a paper read before the Geological Society, which, although the author may claim rather too much for his favourite force, shows in many ways how, by simple gravitation, elevated masses of the earth produce lateral pressure*.

We thus see that while expansion-forces below the surface of the earth produce both lateral compression, uplift, or lateral creep, the effect of gravitation is to intensify the lateral pressure and movement near the surface and to keep the materials closely in contact during and after contraction. Some of the critics of my theory of the origin of mountain-ranges have found a difficulty in understanding how, if the rise of the isogeotherms produces uplift by expansion, the fall of the isogeotherms does not produce an equivalent sinking by contraction and obliterate the range.

The question is really fully answered in the book itself, in which it is shown that the materials of a mountain-range are by the effects of expansion, or a series of expansions, moved towards and piled up into the range, and that they cannot be drawn back and spread out in the areas from which they have moved. Nothing, in fact, can remove them save denudation.

The effect of contraction is to produce normal faulting, and this occurs most largely in the areas from which the materials of the range have travelled. The mode of action is really in one aspect very fairly shown in the experiments on the expansion of metal plates, of which I have given many illustrations. The total decrease of bulk of the section of the earth's crust, out of which the mountain-range has been elaborated, may eventually equal the previous increase which has produced the range, but the *form* is entirely altered. Subsidence may bring the sea back to the foot of the mountains, or, carried further, may make islands of them; but it cannot obliterate them any more than the subsidence of the ground on which a volcano stands can obliterate it.

A mountain-range grows upwards by compression, a volcano principally by accretion; but they both indicate, though in different ways, a redistribution of materials which it takes denuding agencies geological ages to efface.

* Q. J. G. S. November 1888, pp. 783-796.

XXVII. *A Comparison of the Electric Theory of Light and Sir William Thomson's Theory of a Quasi-labile Æther.*
By J. WILLARD GIBBS*.

A REMARKABLE paper by Sir William Thomson, in the November number of the Philosophical Magazine, has opened a new vista in the possibilities of the theory of an elastic æther. Since the general theory of elasticity gives three waves characterized by different directions of displacement for a single wave-plane, while the phenomena of optics show but two, the first point in accommodating any theory to observation is to get rid (absolutely or sensibly) of the third wave. For this end it has been common to make the æther incompressible, or, as it is sometimes expressed, to make the velocity of the third wave infinite. The velocity of the wave of compression becomes, in fact, infinite as the compressibility vanishes. Of course it has not escaped the notice of physicists that we may also get rid of the third wave by making its velocity zero, as may be done by giving certain values to the constants which express the elastic properties of the medium ; but such values have appeared impossible, as involving an unstable state of the medium. The condition of incompressibility, absolute or approximate, has therefore appeared necessary†. This question of instability has now, however, been subjected to a more searching examination, with the result that the instability does not really exist "*provided we either suppose the medium to extend all through boundless space, or give it a fixed containing-vessel as its boundary.*" This renders possible a very simple theory of light, which has been shown to give Fresnel's laws for the intensities of reflected and refracted light and for double refraction, so far as concerns the phenomena which can be directly observed. The displacement in an æolotropic medium is in the same plane passing through the wave-normal, as was supposed by Fresnel ; but its position in that plane is different, being perpendicular to the ray instead of to the wave-normal‡.

* From the American Journal of Science for February 1889.

† It was under this impression that the paper entitled "A Comparison of the Elastic and the Electric Theories of Light with respect to the Law of Double Refraction and the Dispersion of Colours," in the June number of the Amer. Journ. Sci., was written. The conclusions of that paper, except so far as respects the dispersion of colours, will not apply to the new theory.

‡ Sir William Thomson, *loc. cit.* R. T. Glazebrook, Phil. Mag. December 1888.

It is the object of this paper to compare this new theory with the electric theory of light. In the limiting cases (that is, when we regard the velocity of the missing wave in the elastic theory as zero, and in the electric theory as infinite) we shall find a remarkable correspondence between the two theories; the motions of monochromatic light within isotropic or ælotropic media of any degree of transparency or opacity, and at the boundary between two such media, being represented by equations absolutely identical, except that the symbols which denote displacement in one theory denote force in the other, and *vice versâ**. In order to exhibit this correspondence completely and clearly, it is necessary that the fundamental principles of the two theories should be treated with the same generality, and, so far as possible, by the same method. The immediate consequences of the new theory will therefore be deduced with the same generality and essentially by the same method which has been used with reference to the electric theory in a former volume of the *American Journal of Science* (vol. xxv. p. 107).

The elastic properties of the æther, according to the new theory, *in its limiting case*, may be very simply expressed by means of a vector operator, for which we shall use Maxwell's designation. The *curl* of a vector is defined to be another vector so derived from the first that if u, v, w be the rectangular components of the first, and u', v', w' those of its curl,

$$u' = \frac{dw}{dy} - \frac{dv}{dz}, \quad v' = \frac{du}{dz} - \frac{dw}{dx}, \quad w' = \frac{dv}{dx} - \frac{du}{dy}, \quad . \quad . \quad (1)$$

where x, y, z are rectangular coordinates. With this understanding, if the displacement of the æther is represented by the vector \mathfrak{E} , the force exerted upon any element by the surrounding æther will be

$$-B \text{ curl curl } \mathfrak{E} \, dx \, dy \, dz, \quad . \quad . \quad . \quad (2)$$

where B is a scalar (the so-called *rigidity* of the æther) having the same constant value throughout all space, whether ponderable matter is present or not.

Where there is no ponderable matter, this force must be equated to the reaction of the inertia of the æther. This gives, with omission of the common factor $dx \, dy \, dz$,

$$A \ddot{\mathfrak{E}} = -B \text{ curl curl } \mathfrak{E}, \quad . \quad . \quad . \quad (3)$$

where A denotes the density of the æther.

* In giving us a new interpretation of the equations of the electric theory, the author of the new theory has in fact enriched the mathematical theory of physics with something which may be compared to the celebrated *principle of duality* in geometry.

The presence of ponderable matter disturbs the motions of the æther, and renders them too complicated for us to follow in detail. Nor is this necessary; for the quantities which occur in the equations of optics represent average values, taken over spaces large enough to smooth out the irregularities due to the ponderable particles, although very small as measured by a wave-length*. Now the general principles of harmonic motion† show that, to maintain in any element of volume the motion represented by

$$\mathfrak{C} = \mathfrak{A} e^{2\pi i \frac{z}{p}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

\mathfrak{A} being a complex vector-constant, will require a force from outside represented by a complex linear vector-function of \mathfrak{C} ; that is, the three components of the force will be complex linear functions of the three components of \mathfrak{C} . We shall represent this force by

$$B\Psi\mathfrak{C} \, dx \, dy \, dz, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where Ψ represents a complex linear vector-function‡.

If we now equate the force required to maintain the motion in any element to that exerted upon the element by the surrounding æther, we have the equation

$$\Psi\mathfrak{C} = -\text{curl curl } \mathfrak{C}; \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which expresses the general law for the motion of monochromatic light within any sensibly homogeneous medium, and may be regarded as implicitly including the conditions relating to the boundary of two such media, which are necessary for determining the intensities of reflected and refracted light.

* This is in no respect different from what is always tacitly understood in the theory of sound, where the displacements, velocities, and densities considered are always such average values. But in the theory of light it is desirable to have the fact clearly in mind, on account of the two interpenetrating media (imponderable and ponderable), the laws of light not being in all respects the same as they would be for a single homogeneous medium.

† See Lord Rayleigh's 'Theory of Sound,' vol. i. chapters iv., v.

‡ It amounts essentially to the same thing, whether we regard the force as a linear vector-function of \mathfrak{C} or of \mathfrak{C} , since these differ only by the constant factor $-\frac{4\pi^2}{p^2}$. But there are some advantages in expressing

the force as a function of \mathfrak{C} , because the greater part of the force, in the most important cases, is required to overcome the inertia of the æther, and is thus more immediately connected with \mathfrak{C} .

For let u, v, w be the components of \mathfrak{E} ,

$$\begin{array}{llll} v', v', w' & , & , & \text{curl } \mathfrak{E}, \\ u'', v'', w'' & , & , & \text{curl curl } \mathfrak{E}, \end{array}$$

so that

$$\begin{aligned} u' &= \frac{dw}{dy} - \frac{dv}{dz}, & v' &= \frac{du}{dz} - \frac{dw}{dx}, & w' &= \frac{dv}{dx} - \frac{du}{dy}, \\ u'' &= \frac{dw'}{dy} - \frac{dv'}{dz}, & v'' &= \frac{du'}{dz} - \frac{dw'}{dx}, & w'' &= \frac{dv'}{dx} - \frac{du'}{dy}; \end{aligned}$$

and let the interface be perpendicular to the axis of Z . It is evident that if u' or v' is discontinuous at the interface, the value of u'' or v'' becomes in a sense infinite, *i. e.* $\text{curl curl } \mathfrak{E}$, and therefore, by (6), $\Psi\mathfrak{E}$ will be infinite. Now both \mathfrak{E} and Ψ are discontinuous at the interface, but infinite values for $\Psi\mathfrak{E}$ are not admissible. Therefore u' and v' are continuous. Again, if u or v is discontinuous, u' or v' will become infinite, and therefore u'' or v'' . Therefore u and v are continuous. These conditions may be expressed in the most general manner by saying that the components of \mathfrak{E} and $\text{curl } \mathfrak{E}$ parallel to the interface are continuous. This gives four complex scalar conditions, or in all eight scalar conditions, for the motion at the interface, which are sufficient to determine the amplitude and phase of the two reflected and the two refracted rays in the most general case. It is easy, however, to deduce from these four complex conditions two others, which are interesting and sometimes convenient. It is evident from the definitions of w' and w'' that, if u, v, u' , and v' are continuous at the interface, w' and w'' will also be continuous. Now $-w''$ is equal to the component of $\Psi\mathfrak{E}$ normal to the interface. The following quantities are therefore continuous at the interface:—

$$\left. \begin{array}{l} \text{the components parallel to the interface of } \mathfrak{E}, \\ \text{the component normal to the interface of } \Psi\mathfrak{E}, \\ \text{all components of } \dots\dots\dots \text{curl } \mathfrak{E}. \end{array} \right\} \quad (7)$$

To compare these results with those derived from the electrical theory, we may take the general equation of monochromatic light on the electrical hypothesis from a paper in a former volume of Silliman's American Journal. This equation, which with an unessential difference of notation may be written*

$$-\text{Pot } \mathfrak{F} - \nabla Q = 4\pi\Phi\mathfrak{F}, \quad \dots\dots (8)$$

was established by a method and considerations similar to those

* See the Amer. Journ. Sci. vol. xxv. p. 114, equation (12).

which have been used to establish equation (6), except that the ordinary law of electrodynamic induction had the place of the new law of elasticity. \mathfrak{F} is a complex vector representing the electrical displacement as a harmonic function of the time; Φ is a complex linear vector-operator, such that $4\pi\Phi\mathfrak{F}$ represents the electromotive force necessary to keep up the vibration \mathfrak{F} . Q is a complex scalar representing the electrostatic potential; ∇Q the vector of which the three components are

$$\frac{dQ}{dx}, \quad \frac{dQ}{dy}, \quad \frac{dQ}{dz}.$$

Pot denotes the operation by which, in the theory of gravitation, the potential is calculated from the density of matter*. When it is applied, as here, to a vector, the three components of the result are to be calculated separately from the three components of the operand. $-\nabla Q$ is therefore the electrostatic force, and $-\text{Pot } \ddot{\mathfrak{F}}$ the electrodynamic force. In establishing the equation, it was not assumed that the electrical motions are *solenoidal*, or such as to satisfy the so-called "equation of continuity." We may now, however, make this assumption, since it is the extreme case of the electric theory which we are to compare with the extreme case of the elastic.

It results from the definitions of *curl* and ∇ that $\text{curl } \nabla Q = 0$. We may therefore eliminate Q from equation (8) by taking the curl. This gives

$$-\text{curl Pot } \ddot{\mathfrak{F}} = 4\pi \text{curl } \Phi\mathfrak{F}. \quad . \quad . \quad . \quad . \quad (9)$$

Since curl curl and $\frac{1}{4\pi} \text{Pot}$ are inverse operators for solenoidal vectors, we may get rid of the symbol *Pot* by taking the curl again. We thus get

$$-\ddot{\mathfrak{F}} = \text{curl curl } \Phi\mathfrak{F}. \quad . \quad . \quad . \quad . \quad (10)$$

The considerations for the motion at the boundary between different media are easily obtained from the following considerations. $\text{Pot } \ddot{\mathfrak{F}}$ and Q are evidently continuous at the interface. Therefore the components parallel to the interface of ∇Q , and, by (8), of $\Phi\mathfrak{F}$, will be continuous. Again, $\text{curl Pot } \ddot{\mathfrak{F}}$ is continuous at the interface, as appears from the consideration that $\text{curl Pot } \ddot{\mathfrak{F}}$ is the magnetic force due to the electrical motions $\ddot{\mathfrak{F}}$. Therefore, by (9), $\text{curl } \Phi\mathfrak{F}$ is con-

* The symbol $-\text{Pot}$ is therefore equivalent to $4\pi\nabla^{-2}$, as used by Sir William Thomson (with a happy economy of symbols) at the last meeting of the British Association to express the same law of electrodynamic induction, except that the symbol is here used as a vector operator. See 'Nature,' vol. xxxviii. p. 571, *sub. init.*

tinuous. The solenoidal condition requires that the component of \mathfrak{F} normal to the interface shall be continuous.

The following quantities are therefore continuous at the interface :—

$$\left. \begin{array}{l} \text{the components parallel to the interface of } \Phi\mathfrak{F}, \\ \text{the component normal to the interface of } \mathfrak{F}, \\ \text{all components of } \dots\dots\dots \text{curl } \Phi\mathfrak{F}. \end{array} \right\} (11)$$

Of these conditions, the two relating to the normal components of \mathfrak{F} and $\text{curl } \Phi\mathfrak{F}$ are easily shown to result from the other four conditions, as in the analogous case in the elastic theory.

If we now compare in the two theories the differential equations of the motion of monochromatic light, for the interior of a sensibly homogeneous medium, (6) and (10), and the special conditions for the boundary between two such media as represented by the continuity of the quantities (7) and (11), we find that these equations and conditions become identical, if

$$\mathfrak{F} = \Psi\mathfrak{E}, \quad \dots\dots\dots (12)$$

$$\mathfrak{E} = \Phi\mathfrak{F}, \quad \dots\dots\dots (13)$$

$$\Psi = \Phi^{-1}. \quad \dots\dots\dots (14)$$

In other words, the displacements in either theory are subject to the same general and surface conditions as the forces required to maintain the vibrations in an element of volume in the other theory.

To fix our ideas in regard to the signification of Ψ and Φ , we may consider the case of isotropic media, in which these operators reduce to ordinary algebraic quantities, simple or complex. Now the *curl* of any vector necessarily satisfies the solenoidal condition (the so-called “equation of continuity”), therefore by (6) $\Psi\mathfrak{E}$ and \mathfrak{E} will be solenoidal. So also will \mathfrak{F} and $\Phi\mathfrak{F}$ in the electrical theory. Now for solenoidal vectors,

$$-\text{curl curl} = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}; \quad \therefore \quad (15)$$

so that the equations (6) and (10) reduce to

$$\Psi\ddot{\mathfrak{E}} = \left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} \right) \mathfrak{E}, \quad \dots\dots\dots (16)$$

$$\ddot{\mathfrak{F}} = \left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} \right) \Phi\mathfrak{F}. \quad \dots\dots\dots (17)$$

For a simple train of waves, the displacement, in either theory, may be represented by a constant multiplied by

$$e^{i(gt+ax+by+cz)} \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \quad (18)$$

Our equations then reduce again to

$$g^2\Psi(\mathfrak{F})=(a^2+b^2+c^2)\mathfrak{F}, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

$$g^2\mathfrak{F}=(a^2+b^2+c^2)\Phi\mathfrak{F}. \quad . \quad . \quad . \quad . \quad (20)$$

Hence

$$\Psi^{-1} = \Phi = \frac{g^2}{a^2 + b^2 + c^2}. \quad . \quad . \quad . \quad . \quad (21)$$

The last member of this equation, when real, evidently expresses the square of the velocity of light. If we set

$$n^2 = k^2 \frac{a^2 + b^2 + c^2}{q^2}, \quad . \quad . \quad . \quad . \quad . \quad (22)$$

k denoting the velocity of light *in vacuo*, we have

$$n^2 = k^2 \Psi = k^2 \Phi^{-1}. \quad (23)$$

When n^2 is positive, which is the case of perfectly transparent bodies, the positive root of n^2 is called the index of refraction of the medium. In the most general case it would be appropriate to call n (or perhaps that root of n^2 of which the real part is positive) the (complex) index of refraction, although the terminology is hardly settled in this respect. A negative value of n^2 would represent a body from which light would be totally reflected at all angles of incidence. No such cases have been observed. Values of n^2 in which the coefficient of ϵ is negative, indicate media in which light is absorbed. Values in which the coefficient of ϵ is positive would represent media in which the opposite phenomenon took place*.

It is no part of the object of this paper to go into the details by which we may derive, so far as observable phenomena are concerned, Fresnel's law of double refraction for transparent bodies, as well as the more general law of the same character which relates to æolotropic bodies of more or less opacity, and which differs from Fresnel's only in that certain quantities become complex, or Fresnel's laws for the intensities of reflected and refracted light at the boundary of transparent isotropic media with the more general laws for the case of bodies æolotropic or opaque, or both. The principal cases have already been discussed on the new elastic theory

* But ι might have been introduced into the equations in such a way that a positive coefficient in the value of n^2 would indicate absorption, and a negative coefficient the impossible case.

in the Philosophical Magazine*, and a further discussion is promised. For the electrical theory, the case of double refraction in perfectly transparent media has been discussed quite in detail in the Amer. Journ. Sci.†; and the intensities of reflected and refracted light have been abundantly deduced from the above conditions by various authors‡. So far as all these laws are concerned, the object of this paper will be attained if it has been made clear that the two theories, in their extreme cases, give identical results. The greater or less degree of elegance, or completeness, or perspicuity, with which these laws may be developed by different authors should weigh nothing in favour of either theory.

The nonmagnetic rotation of the plane of polarization, with the allied phenomena in ælotropic bodies, lie in a certain sense outside of the above laws, as depending on minute quantities which have been neglected in this discussion. The manner in which these minute quantities affect the equations of motion on the electrical theory has been shown in a former paper§, where these phenomena in transparent bodies are treated quite at length. For the new theory, a discussion of this subject is promised by Mr. Glazebrook.

But the magnetic rotation of the plane of polarization, with the allied phenomena when an ælotropic body is subjected to magnetic influence, fall entirely within the scope of the above equations and surface-conditions. The characteristic of this case is that Ψ and Φ are not self-conjugate||. This is what we might expect on the electric theory from the experiments of Dr. Hall, which show that the operators expressing the relation between the electromotive force and current are not in general self-conjugate in this case.

In the preceding comparison, we have considered only the limiting cases of the two theories. With respect to the sense in which the limiting case is admissible, the two theories do not stand on quite the same footing. In the electric theory, or in any in which the velocity of the missing wave is very great, if we are satisfied that the compressibility is so small as to produce no appreciable results, we may set it equal to zero

* Sir William Thomson, *loc. cit.* R. T. Glazebrook, *loc. cit.*

† Vol. xxiii. p. 262.

‡ Lorentz, Schlömilch's *Zeitschrift*, vol. xxii. pp. 1-30 and 205-219; vol. xxiii. pp. 197-210; Fitzgerald, *Phil. Trans.* vol. clxxi. p. 691; J. J. Thomson, *Phil. Mag.* [5] vol. ix. p. 284; Rayleigh, *Phil. Mag.* [5] vol. xii. p. 81. Glazebrook, *Proc. Camb. Phil. Soc.* vol. iv. p. 155.

§ *Amer. Journ. Sci.* vol. xxiii. p. 460.

|| See *Amer. Journ. Sci.* vol. xxv. p. 113.

in our mathematical theory, even if we do not regard this as expressing the actual facts with absolute accuracy. But the case is not so simple with an elastic theory in which the forces resisting certain kinds of motion vanish, so far, at least, as they are proportional to the strains. The first requisite for any sort of optical theory is that the forces shall be proportional to the displacements. This is easily obtained in general by supposing the displacements very small. But if the resistance to one kind of distortion vanishes, there will be a tendency for this kind of distortion to appear in some places in an exaggerated form, and even to an infinite degree, however small the displacements may be in other parts of the field. In the case before us, if we suppose the velocity of the missing wave to be absolutely zero, there will be infinite condensations and rarefactions at a surface where ordinary waves are reflected; that is, a certain volume of æther will be condensed to a surface, and *vice versâ*. This prevents any treatment of the extreme case, which is at once simple and satisfactory. The difficulty has been noticed by Sir William Thomson, who observes that it may be avoided if we suppose the displacements infinitely small in comparison with the wave-length of the wave of compression. This implies a finite velocity for that wave. A similar difficulty would probably be found to exist (in the extreme case) with regard to the deformation of the æther by the molecules of ponderable matter, as the æther oscillates among them. If the statical resistance to irrotational motions is zero, it is not at all evident that the statical forces evoked by the disturbance caused by the molecules would be proportional to the motions. But this difficulty would be obviated by the same hypothesis as the first.

These circumstances render the elastic theory somewhat less convenient as a working hypothesis than the electric. They do not necessarily involve any complication of the equations of optics. For it may still be possible that this velocity of the missing wave is so small, that the quantities on which it depends may be set equal to zero in the equations which represent the phenomena of optics. But the mental processes by which we satisfy ourselves of the validity of our results (if we do not work out the whole problem in the general case of no assumption in regard to the velocity of the missing wave) certainly involve conceptions of a higher degree of difficulty on account of the circumstances mentioned. Perhaps this ought not to affect our judgment with respect to the question of the truth of the hypothesis.

Although the two theories give laws of exactly the same form for monochromatic light in the limiting case, their devia-

tions from this limit are in opposite directions ; so that if the phenomena of optics differed in any marked degree from what we would have in the limiting case, it would be easy to find an *experimentum crucis* to decide between the two theories. A little consideration will make it evident that, when the principal indices of refraction of a crystal are given, the intermediate values for oblique wave-planes will be less if the velocity of the missing wave is small but finite than if it is infinitesimal, and will be greater if the velocity of the missing wave is very great but finite than if it is infinite*. Hence, if the velocity of the missing wave is small but finite, the intermediate values of the indices of refraction will be less than are given by Fresnel's law ; but if the velocity of the missing wave is very great but finite, the intermediate values of the indices of refraction will be greater than are given by Fresnel's law. But the recent experiments of Professor Hastings on the law of double refraction in Iceland spar do not encourage us to look in this direction for the decision of the question †.

In a simple train of waves in a transparent medium, the potential energy, on the elastic theory, may be divided into two parts ; of which one is due to that general deformation of the æther which is represented by the equations of wave-motion, and the other to those deformations which are caused by the interference of the ponderable particles with the wave-motion, and to such displacements of the ponderable matter as may be caused, in some cases at least, by the motion of the æther. If we write h for the amplitude, l for the wave-length, and p for the period, these two parts of the statical energy (estimated per unit volume for a space including many wave-lengths) may be represented respectively by

$$\frac{\pi^2 B h^2}{l^2} \text{ and } \frac{b h^2}{4}.$$

The sum of these may be equated to the kinetic energy, giving an equation of the form

$$\frac{\pi^2 B h^2}{l^2} + \frac{b h^2}{4} = \frac{\pi^2 A' h^2}{p^2} (24)$$

* This may be more clear if we consider the stationary waves formed by two trains of waves moving in opposite directions. The case then comes under the following theorem :—

“If the system undergo such a change that the potential energy of a given configuration is diminished, while the kinetic energy of a given motion is unaltered, the periods of the free vibrations are all increased, and conversely.” See Lord Rayleigh's ‘Theory of Sound,’ vol. i. p. 85.

† Amer. Journ. Sci. vol. xxxiii. p. 60.

B is an absolute constant (the rigidity of the æther, previously represented by the same letter), A' and b will be constant (for the same medium and the same direction of the wave-normal) except so far as the type of the motion changes; *i. e.* except so far as the manner in which the motion of the æther distributes itself between the ponderable molecules, and the degree in which these take part in the motion, may undergo a change. When the period of vibration varies, the type of motion will vary more or less, and A' and b will vary more or less.

In a manner entirely analogous*, the *kinetic* energy, on the electrical theory, may be divided into two parts; of which one is due to those general fluxes which are represented by the equations of wave motions, and the other to those irregularities in the fluxes which are caused by the presence of the ponderable molecules, as well as to such motions of the ponderable particles themselves, as may sometimes occur. These parts of the kinetic energy may be represented respectively by

$$\frac{\pi F l^2 h^2}{p^2} \quad \text{and} \quad \frac{\pi^2 f h^2}{p^2}.$$

Their sum equated to the potential energy gives

$$\frac{\pi F l^2 h^2}{p^2} + \frac{\pi^2 f h^2}{p^2} = \frac{G h^2}{4}. \quad . \quad . \quad . \quad . \quad (25)$$

Here F is the constant of electrodynamic induction, which is unity if we use the electromagnetic system of units; f and G (like A' and b) vary only so far as the type of motion varies.

We have the means of forming a very exact numerical estimate of the ratio of the two parts into which the statical energy is thus divided on the elastic theory, or the kinetic energy on the electric theory. The means for this estimate is afforded by the principle, that the period of a natural vibration is stationary when its type is infinitesimally altered by any constraint†. Let us consider a case of simple wave-motion, and suppose the period to be infinitesimally varied, the wave-length will also vary, and presumably to some extent the type of vibration. But, by the principle just stated, if the æther or the electricity could be constrained to vibrate in the original type, the variations of l and p would be the same as in the actual case. Therefore, in finding the differential equation between l and p , we may treat b and A' in (24) and

* See Amer. Journ. Sci. vol. xxii. p. 262.

† See Lord Rayleigh's 'Theory of Sound,' vol. i. p. 84. The application of the principle is most simple in the case of stationary waves.

f and G in (25) as constant, as well as B and F . These equations may be written

$$4\pi^2 B \frac{p^2}{l^2} + bp^2 = 4\pi^2 A',$$

$$\pi F \frac{l^2}{p^2} + \frac{\pi^2 f}{p^2} = \frac{1}{4} G.$$

Differentiating, we get

$$4\pi^2 B d \frac{p^2}{l^2} = -bd(p^2),$$

$$\pi F d \frac{l^2}{p^2} = -\pi^2 f d(p^{-2});$$

or

$$4\pi^2 B \frac{p^2}{l^2} d \log \frac{p^2}{l^2} = -bp^2 d \log p^2,$$

$$\pi F \frac{l^2}{p^2} d \log \frac{l^2}{p^2} = -\frac{\pi^2 f}{p^2} d \log p^{-2}.$$

Hence, if we write V for the wave-velocity (l/p), n for the index of refraction, and λ for the wave-length *in vacuo*, we have for the ratio of the two parts into which we have divided the potential energy on the elastic theory,

$$\frac{bh^2}{4} \div \frac{\pi^2 Bh^2}{l^2} = \frac{d \log V}{d \log p} = -\frac{d \log n}{d \log \lambda}; \quad \dots \quad (26)$$

and for the ratio of the two parts into which we have divided the kinetic energy on the electrical theory,

$$\pi^2 \frac{fh^2}{p^2} \div \frac{\pi Fl^2 h^2}{p^2} = \frac{d \log V}{d \log p} = -\frac{d \log n}{d \log \lambda}. \quad \dots \quad (27)$$

It is interesting to see that these ratios have the same value. This value may be expressed in another form, which is suggestive of some important relations. If we write U for what Lord Rayleigh has called the velocity of a group of waves*,

$$\left. \begin{aligned} \frac{U}{V} &= 1 - \frac{d \log V}{d \log l}, \\ \frac{d \log V}{d \log l} &= \frac{V-U}{V}, \\ \frac{d \log V}{d \log p} &= \frac{V-U}{U}. \end{aligned} \right\} \quad \dots \quad (28)$$

* See his "Note on Progressive Waves," Proc. Lond. Math. Soc. vol. ix. No. 125, reprinted in his 'Theory of Sound,' vol. ii. p. 297.

It appears, therefore, that in the elastic theory that part of the potential energy which depends on the deformation expressed by the equations of wave-motion bears to the whole potential energy the same ratio which the velocity of a group of waves bears to the wave-velocity. In the electrical theory, that part of the kinetic energy which depends on the motions expressed by the equations of wave-motion bears to the whole kinetic energy the same ratio.

Returning to the consideration of equations (26) and (27), we observe that in transparent bodies the last member of these equations represents a quantity which is small compared with unity, at least in the visible spectrum, and diminishes rapidly as the wave-length increases. This is just what we should expect of the first member of equation (27). But when we pass to equation (26), which relates to the elastic theory, the case is entirely different. The fact that the kinetic energy is affected by the presence of the ponderable matter, and affected differently in different directions, shows that the motion of the æther is considerably modified. This implies a distortion superposed upon the distortion represented by the equations of wave-motion, and very much greater, since the body is very fine-grained as measured by a wave-length. With any other law of elasticity we should suppose that the energy of this superposed distortion would enormously exceed that of the regular distortion represented by the equations of wave-motion. But it is the peculiarity of this new law of elasticity that there is one kind of distortion of which the energy is very small, and which is therefore peculiarly likely to occur. Now if we can suppose the distortion caused by the ponderable molecules to be almost entirely of this kind, we may be able to account for the smallness of its energy. We should still expect the first member of (26) to increase with the wave-length on account of the factor l^2 , instead of diminishing, as the last member of the equation shows that it does. We are obliged to suppose that b , and therefore the type of the vibrations, varies very rapidly with the wave-length, even in those cases which appear farthest removed from anything like selective absorption.

The electrical theory furnishes a relation between the refractive power of a body and its specific dielectric capacity, which is commonly expressed by saying that the latter is equal to the square of the index of refraction for waves of infinite length. No objection can be made to this statement; but the great uncertainty in determining the index for waves of infinite length by extrapolation prevents it from furnishing any very rigorous test of the theory. Yet as the results of extra-

polarisation in some cases agree strikingly with the specific dielectric capacity, although in other cases they are quite different, the correspondence is generally regarded as corroborative, in some degree, of the theory. But the relation between refractive power and dielectric capacity may be expressed in a form which will furnish a more rigorous test, as not involving extrapolation.

We have seen on page 249 how we may determine numerically the ratio of the two first terms of equation (25). We thus easily get the ratio of the first and last term, which gives

$$\frac{Gh^2}{4} = \frac{d \log l}{d \log \lambda} \frac{\pi F l^2 h^2}{p^2} \dots \dots \dots (29)$$

In the corresponding equation for a train of waves of the same amplitude and period *in vacuo*, l becomes λ , F remains the same, and for G we may write G' . This gives

$$\frac{G'h^2}{4} = \frac{\pi F \lambda^2 h^2}{p^2} \dots \dots \dots (30)$$

Dividing, we get

$$\frac{G}{G'} = \frac{d \log l}{d \log \lambda} \frac{l^2}{\lambda^2} = \frac{d(l^2)}{d(\lambda^2)} \dots \dots \dots (31)$$

Now G' is the dielectric elasticity of pure æther. If K is the specific dielectric capacity of the body which we are considering, G'/K is the dielectric elasticity of the body and $G'/2K$ is the potential energy of the body (per unit of volume), due to a unit of ordinary electrostatic displacement. But $Gh^2/4$ is the potential energy in a train of waves of amplitude h . Since the average square of the displacement is $h^2/2$, the potential energy of a unit displacement such as occurs in a train of waves is $G/2$. Now in the electrostatic experiment the displacement distributes itself among the molecules so as to make the energy a minimum. But in the case of light the distribution of the displacement is not determined entirely by statical considerations. Hence

$$\left. \begin{aligned} \frac{G}{2} &\geq \frac{G'}{2K}, \\ K &\geq \frac{G'}{G}, \end{aligned} \right\} \dots \dots \dots (32)$$

and

$$K \geq \frac{d(\lambda^2)}{d(l^2)} \dots \dots \dots (33)$$

It is to be observed that if we should assume for a dispersion-formula

$$n^{-2} = a - b\lambda^{-2}, \dots \dots \dots (34)$$

$1/a$, which is the square of the index of refraction for an infinite wave-length, would be identical with the second member of (33).

Another similarity between the electrical and optical properties of bodies consists in the relation between conductivity and opacity. Bodies in which electrical fluxes are attended with absorption of energy absorb likewise the energy of the motions which constitute light. This is strikingly true of the metals. But the analogy does not stop here. To fix our ideas, let us consider the case of an isotropic body and circularly polarized light, which is geometrically the simplest case, although its analytical expression is not so simple as that of plane-polarized light. The displacement at any point may be symbolized by the rotation of a point in a circle. The external force necessary to maintain the displacement \mathfrak{F} is represented by $n^{-2}\mathfrak{F}$. In transparent bodies, for which n^{-2} is a positive number, the force is radial and in the direction of the displacement, being principally employed in counterbalancing the dielectric elasticity, which tends to diminish the displacement. In a conductor n^{-2} becomes complex, which indicates a component of the force in the direction of \mathfrak{F} , that is, tangential to the circle. This is only the analytical expression of the fact above mentioned. But there is another optical peculiarity of metals, which has caused much remark, viz. that the real part of n^2 (and therefore of n^{-2}) is negative, *i. e.* the radial component of the force is directed towards the centre. This inwardly directed force, which evidently opposes the electrodynamic induction of the irregular part of the motion, is small compared with the outward force which is found in transparent bodies, but increases rapidly as the period diminishes. We may say, therefore, that metals exhibit a second optical peculiarity—that the dielectrical elasticity is not prominent as in transparent bodies. This is like the electrical behaviour of the metals, in which we do not observe any elastic resistance to the motion of electricity. We see, therefore, that the complex indices of metals, both in the real and imaginary part of their inverse squares, exhibit properties corresponding to the electrical behaviour of the metals.

The case is quite different in the elastic theory. Here the force from outside necessary to maintain in any element of volume the displacement \mathfrak{C} is represented by $n^2\mathfrak{C}$. In transparent bodies, therefore, it is directed toward the centre. In metals, there is a component in the direction of the motion \mathfrak{C} , while the radial part of the force changes its direction and is often many times greater than the opposite force in transparent bodies. This indicates that in metals the displacement of

the æther is resisted by a strong elastic force, quite enormous compared to anything of the kind in transparent bodies, where it indeed exists, but it is so small that it has been neglected by most writers, except when treating of dispersion. We can make these suppositions, but they do not correspond to anything which we know independently of optical experiment.

It is evident that the electrical theory of light has a serious rival, in a sense in which, perhaps, one did not exist before the publication of Sir William Thomson's paper in November last*. Nevertheless, neither surprise at the results which have been achieved, nor admiration for that happy audacity of genius, which, seeking the solution of the problem precisely where no one else would have ventured to look for it, has turned half a century of defeat into victory, should blind us to the actual state of the question.

It may still be said for the electrical theory, that it is not obliged to invent hypotheses†, but only to apply the laws furnished by the science of electricity, and that it is difficult to account for the coincidences between the electrical and optical properties of media, unless we regard the motions of light as electrical. But if the electrical character of light is conceded, the optical problem is very different from anything which existed in the time of Fresnel, Cauchy, and Green. The third wave, for example, is no longer something to be gotten rid of *quocunque modo*, but something which we must dispose of in accordance with the laws of electricity. This would seem to rule out the possibility of a relatively small velocity for the third wave.

* "Since the first publication of Cauchy's work on the subject in 1830, and of Green's in 1837, many attempts have been made by many workers to find a dynamical foundation for Fresnel's laws of reflexion and refraction of light, but all hitherto ineffectually." Sir William Thomson, *loc. cit.*

"So far as I am aware, the electric theory of Maxwell is the only one satisfying these conditions (of explaining at once Fresnel's laws of double refraction in crystals and those governing the intensity of reflexion when light passes from one isotropic medium to another)." Lord Rayleigh, *Phil. Mag.* September 1888.

† Electrical motions in air, since the recent experiments of Professor Hertz, seem to be no longer a matter of hypothesis. We can hardly suppose that the case is essentially different with the so-called vacuum. The theorem that the electrical motions of light are solenoidal, although it is convenient to assume it as a hypothesis and show that the results agree with experiment, need not occupy any such fundamental position in the theory. It is in fact only another way of saying that two of the constants of electrical science have a certain ratio (infinity). It would be easy to commence without assuming this value, and to show in the course of the development of the subject that experiment requires it, not of course as an abstract proposition, but in the sense in which experiment can be said to require any values of any constants, that is, to a certain degree of approximation.

XXVIII. *On the Oscillations of a Rotating Liquid Spheroid and the Genesis of the Moon.* By A. E. H. LOVE, B.A.*

1. RIEMANN'S investigations † of the motion of a liquid ellipsoid contain the condition of stability of the form of steady motion usually referred to as Maclaurin's spheroid, when the liquid is perfectly inviscid. The equation for the critical value of the excentricity of the spheroid is

$$e(3+4e^2) \sqrt{1-e^2} = (3+2e^2-4e^4) \sin^{-1} e;$$

and, if we take the notation of Thomson and Tait in which $(1+f^2)(1-e^2)=1$, the critical value of f is very nearly 3·14. If f be greater than this value the motion is unstable. If the liquid be viscous, it is stated by Thomson and Tait that the motion is secularly unstable, however slight the viscosity may be, if f exceeds 1·39457. This statement has been recently proved by Poincaré ‡, who has also shown that the motion is thoroughly stable for all displacements when f is less than this value.

In this paper is given an investigation by the method of Greenhill and Basset of the equations, first obtained by Riemann, determining the lengths of the axis of a liquid ellipsoid which rotates about one of its principal axes and moves in such a way as to remain ellipsoidal; and these equations are then applied to find the small oscillations about that state of steady motion in which the free surface is an oblate spheroid, and the liquid rotates as if rigid about the polar axis; the displacement contemplated being of such a kind that the axis of rotation remains fixed in space, and the surface is always ellipsoidal and has this axis for one of its principal axes. It appears that there are two periods of oscillation, $2\pi/n_1$ and $2\pi/n_2$, where

$$n_1^2 = 16\pi\gamma\rho / (3+f^2) - 2\omega^2(3+8f^2+f^4)/(3f^2+f^4)$$

and

$$n_2^2 = 24\pi\gamma\rho(1+f^2)/(3+f^2)^2 - \omega^2(1+f^2)(27+18f^2-f^4)/(3+f^2)^2.$$

In these ρ is the density of the liquid, ω the angular velocity, and γ the constant of gravitation, and ω and f are connected

* Communicated by the Author, having been read before the British Association, September 6, 1888.

† *Abh. kön. Ges. Wiss. Gött.* 1860.

‡ *Acta Mathematica*, vii. (1885).

by the condition of steady motion,

$$(3 + f^2) \tan^{-1} f = f(3 + \omega^2 f^2 / 2\pi\gamma\rho).$$

These values of n vary very little for different small values of ω , so that for a spheroid rotating in any period longer than about three hours the period of oscillation varies inversely as the square root of the density approximately.

The determination of these periods of oscillation has an important bearing on the question of the origin of the Moon. Professor Darwin, in his paper "On the Precession of a Viscous Spheroid and the Remote History of the Earth,"* saw reason to reject Laplace's hypothesis, that the moon separated from the earth as a ring because the angular velocity was too great for stability. In the light of Riemann and Poincaré's researches above referred to it is clear that, when the density is not less than 3 and the period of rotation longer than three hours, the motion is certainly stable. According to Professor Darwin, the period of rotation of the earth-moon system when the two bodies formed a single mass may be estimated at something between two and six hours, more probably between two and four hours†; and if we take account of the continued contraction of the two cooling bodies since the date of the disruption and remember that the present mean density of the moon is about 3.7, it seems highly improbable that Laplace's hypothesis as to instability can be correct. As an alternative Professor Darwin has suggested that possibly the spheroid might have a period of free oscillation not far removed from the semidiurnal tidal period, in which case the solar tides would be of enormous height. This is a new cause of instability in the otherwise stable dynamical system, and its consequence would be a division of the mass at a moment of greatest elongation. Thus on this hypothesis the moon broke off from the earth as a single mass and not as a ring, and we have not in our account of the history of the system to make additional demands upon the lapse of time with a view to the consolidation of the ring-moon into one body. The hypothesis gains greatly in credibility when it is shown that the spheroid really has a period of oscillation of the requisite length. This is done in the present paper. It is proved that for a liquid spheroid of the same mean density as the earth the longest period is always very nearly equal to $1\frac{1}{2}$ hours, while for a spheroid whose density is 3 this period is very nearly equal to 2 hours, whatever the rate of rotation

* Phil. Trans. (1879).

† "On the Secular Changes in the Elements of the Orbit of a Satellite," § 22, Phil. Trans. 1880.

may be, provided it is not faster than once in 3 hours ; so that if the length of the day was ever about 4 hours the density must have been such that, as the spheroid contracted, and rotated faster, the period of free oscillation coincided with that of the semidiurnal tide before the length of the day was 3 hours.

2. Suppose a mass of liquid enclosed in an ellipsoidal case, whose equation is $x^2/a^2 + y^2/b^2 + z^2/c^2 = 1$, to be rotating as if rigid about the axis z with angular velocity ζ , and let an additional angular velocity Ω about the same axis be imparted to the case, and let the case be made to change form but so as to remain ellipsoidal and of constant volume ; the velocity-potentials of the motions set up in the liquid by these two motions of the case are

$$\Omega \frac{a^2 - b^2}{a^2 + b^2} xy,$$

and

$$\frac{1}{2} \left(\frac{\dot{a}}{a} x^2 + \frac{\dot{b}}{b} y^2 + \frac{\dot{c}}{c} z^2 \right),$$

x, y, z being the coordinates of a particle of the fluid referred to the principal axes of the ellipsoid at time t , these axes rotating with angular velocity $\omega = \zeta + \Omega$. The velocity-components of the fluid-element which at time t is at (x, y, z) are

$$\left. \begin{aligned} u &= \frac{\dot{a}}{a} x - \zeta y + \Omega \frac{a^2 - b^2}{a^2 + b^2} y, \\ v &= \frac{\dot{b}}{b} y + \zeta x + \Omega \frac{a^2 - b^2}{a^2 + b^2} x, \\ w &= \frac{\dot{c}}{c} z. \end{aligned} \right\} \quad . \quad . \quad . \quad (1)$$

If U, V, W be the rates of change of the coordinates of a fluid particle at (x, y, z) , we shall have

$$\left. \begin{aligned} U &= u + \omega y = \frac{\dot{a}}{a} x + \frac{2a^2}{a^2 + b^2} \Omega y, \\ V &= v - \omega x = \frac{\dot{b}}{b} y - \frac{2b^2}{a^2 + b^2} \Omega x, \\ W &= w = \frac{\dot{c}}{c} z. \end{aligned} \right\} \quad . \quad . \quad . \quad (2)$$

3. The Eulerian equations referred to the moving axes are

$$\left. \begin{aligned} \frac{\partial u}{\partial t} - \omega v + U \frac{\partial u}{\partial x} + V \frac{\partial u}{\partial y} + w \frac{\partial u}{\partial z} &= \frac{\partial}{\partial x} \left(V_1 - \frac{p}{\rho} \right), \\ \frac{\partial v}{\partial t} + \omega u + U \frac{\partial v}{\partial x} + V \frac{\partial v}{\partial y} + w \frac{\partial v}{\partial z} &= \frac{\partial}{\partial y} \left(V_1 - \frac{p}{\rho} \right), \\ \frac{\partial w}{\partial t} + U \frac{\partial w}{\partial x} + V \frac{\partial w}{\partial y} + w \frac{\partial w}{\partial z} &= \frac{\partial}{\partial z} \left(V_1 - \frac{p}{\rho} \right); \end{aligned} \right\} \quad (3)$$

where p is the pressure, ρ the density, and V_1 the potential of the bodily forces.

Now

$$\begin{aligned} \frac{\partial u}{\partial t} = x \left(\frac{\ddot{a}}{a} - \frac{\dot{a}^2}{a^2} \right) + y \left[-\dot{\zeta} + \frac{a^2 - b^2}{a^2 + b^2} \dot{\Omega} \right. \\ \left. + \Omega \left\{ \frac{\dot{a}}{a} \frac{4a^2b^2}{(a^2 + b^2)^2} - \frac{b}{b} \frac{4a^2b^2}{(a^2 + b^2)^2} \right\} \right]; \end{aligned}$$

so that

$$\begin{aligned} \frac{\partial}{\partial x} \left(V_1 - \frac{p}{\rho} \right) = x \left[\left(\frac{\ddot{a}}{a} - \frac{\dot{a}^2}{a^2} \right) - \omega \left(\zeta + \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) + \frac{\dot{a}}{a} \left(\frac{\dot{a}}{a} \right) \right. \\ \left. + \frac{2b^2}{a^2 + b^2} \Omega \left(\zeta - \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) \right] \\ + y \left[-\dot{\zeta} + \frac{a^2 - b^2}{a^2 + b^2} \dot{\Omega} + \frac{4a^2b^2}{(a^2 + b^2)^2} \Omega \left(\frac{\dot{a}}{a} - \frac{\dot{b}}{b} \right) - \omega \frac{\dot{b}}{b} \right. \\ \left. + \frac{2a^2}{a^2 + b^2} \Omega \frac{\dot{a}}{a} - \left(\zeta - \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) \frac{\dot{b}}{b} \right] \\ = a'x + h'y \text{ say.} \quad \dots \dots \dots (4) \end{aligned}$$

In like manner,

$$\begin{aligned} \frac{\partial}{\partial y} \left(V_1 - \frac{p}{\rho} \right) = x \left[\dot{\zeta} + \frac{a^2 - b^2}{a^2 + b^2} \dot{\Omega} + \frac{4a^2b^2}{(a^2 + b^2)^2} \Omega \left(\frac{\dot{a}}{a} - \frac{\dot{b}}{b} \right) \right. \\ \left. + \omega \frac{\dot{a}}{a} - \frac{2b^2}{a^2 + b^2} \Omega \frac{\dot{b}}{b} + \left(\zeta + \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) \frac{\dot{a}}{a} \right] \\ + y \left[\left(\frac{\ddot{b}}{b} - \frac{\dot{b}^2}{b^2} \right) - \omega \left(\zeta - \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) + \frac{\dot{b}}{b} \left(\frac{\dot{b}}{b} \right) \right. \\ \left. + \frac{2a^2}{a^2 + b^2} \Omega \left(\zeta + \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) \right] \\ = h''x + b'y \text{ say.} \quad \dots \dots \dots (5) \end{aligned}$$

$$\begin{aligned} \frac{\partial}{\partial z} \left(V_1 - \frac{p}{\rho} \right) = \frac{\ddot{c}}{c} z \\ = c'z \text{ say.} \quad \dots \dots \dots (6) \end{aligned}$$

By Helmholtz's equation of vortex motion, ζ being independent of x, y, z ,

$$\dot{\zeta} = \zeta \frac{\partial w}{\partial z} = \zeta \frac{\dot{c}}{c} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Hence

$$h' = -\zeta \left(\frac{2\dot{b}}{b} + \frac{\dot{c}}{c} \right) + \frac{1}{(a^2 - b^2)} \frac{d}{dt} \left\{ \Omega \frac{(a^2 - b^2)^2}{a^2 + b^2} \right\},$$

and

$$h'' = \zeta \left(\frac{2\dot{a}}{a} + \frac{\dot{c}}{c} \right) + \frac{1}{(a^2 - b^2)} \frac{d}{dt} \left\{ \Omega \frac{(a^2 - b^2)^2}{a^2 + b^2} \right\},$$

so that

$$h' = h'' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Now $a'x + h'y$ is the acceleration of the particle at (x, y, z) parallel to the axis x , and $h'x + b'y$ is the acceleration parallel to the axis y , and thus the moment of the effective forces about the axis z is proportional to h' . Hence $h' = 0$.

Now, by integration,

$$V_1 - p/\rho = \frac{1}{2} (a'x^2 + b'y^2 + c'z^2) + \text{const.} \quad . \quad . \quad (9)$$

Suppose the liquid to be subject to its own attraction; then

$$V_1 = \pi \gamma \rho abc \int_0^\infty \frac{d\psi}{P} \left(1 - \frac{x^2}{a^2 + \psi} - \frac{y^2}{b^2 + \psi} - \frac{z^2}{c^2 + \psi} \right), \quad (10)$$

where

$$P^2 = (a^2 + \psi)(b^2 + \psi)(c^2 + \psi),$$

or

$$V_1 = \gamma \rho \left[H - \frac{1}{2} (Ax^2 + By^2 + Cz^2) \right]. \quad . \quad . \quad . \quad (11)$$

Hence the pressure is given by

$$p/\rho = \text{const.} - \frac{1}{2} [(\gamma \rho A + a')x^2 + (\gamma \rho B + b')y^2 + (\gamma \rho C + c')z^2]. \quad (12)$$

The surface $x^2/a^2 + y^2/b^2 + z^2/c^2 = 1$ can be free if

$$\frac{1}{2} (\gamma \rho A + a')a^2 = \frac{1}{2} (\gamma \rho B + b')b^2 = \frac{1}{2} (\gamma \rho C + c')c^2 = \sigma \text{ say,} \quad (13)$$

and then the pressure at any point is given by

$$p/\rho = P_1/\rho + \sigma (1 - x^2/a^2 - y^2/b^2 - z^2/c^2). \quad . \quad . \quad (14)$$

4. Now

$$a' = \ddot{a} - (\zeta + \Omega) \left(\zeta + \Omega \frac{a^2 - b^2}{a^2 + b^2} \right) + \frac{2b^2}{a^2 + b^2} \Omega \left(\zeta - \Omega \frac{a^2 - b^2}{a^2 + b^2} \right),$$

or

$$aa' = \ddot{a} - \zeta^2 a - 2\zeta \Omega a \frac{a^2 - b^2}{a^2 + b^2} - \Omega^2 a \frac{a^2 - 3b^2}{(a^2 + b^2)^2},$$

$$= \ddot{a} - \frac{1}{2} \left[(a - b) \left(\zeta + \Omega \frac{(a + b)^2}{a^2 + b^2} \right)^2 + (a + b) \left(\zeta + \Omega \frac{(a - b)^2}{a^2 + b^2} \right)^2 \right],$$

so

$$bb' = \ddot{b} - \frac{1}{2} \left[(b-a) \left(\zeta + \Omega \frac{(a+b)^2}{a^2+b^2} \right)^2 + (b+a) \left(\zeta + \Omega \frac{(a-b)^2}{a^2+b^2} \right)^2 \right].$$

Hence the equations of motion as given by (13) become

$$\left. \begin{aligned} \frac{\tau^2}{(a-b)^3} + \frac{\tau'^2}{(a+b)^3} - \frac{1}{2} \ddot{a} &= \frac{1}{2} \gamma \rho a A - \frac{\sigma}{a}, \\ \frac{\tau^2}{(b-a)^3} + \frac{\tau'^2}{(b+a)^3} - \frac{1}{2} \ddot{b} &= \frac{1}{2} \gamma \rho b B - \frac{\sigma}{b}, \\ -\frac{1}{2} \ddot{c} &= \frac{1}{2} \gamma \rho c C - \frac{\sigma}{c}, \end{aligned} \right\} \quad (14a^*)$$

where

$$\tau = \frac{1}{2} (a-b)^2 \left[\zeta + \Omega \frac{(a+b)^2}{(a^2+b^2)} \right],$$

$$\tau' = \frac{1}{2} (a+b)^2 \left[\zeta + \Omega \frac{(a-b)^2}{a^2+b^2} \right].$$

We can show that τ, τ' are constants, by forming the equations of conservation of moment of momentum and of vortex-strength.

The moment of momentum round the axis z is

$$\iiint \left\{ x^2 \left(\zeta + \Omega \frac{a^2-b^2}{a^2+b^2} \right) + y^2 \left(\zeta - \Omega \frac{a^2-b^2}{a^2+b^2} \right) \right\} dx dy dz = \text{const.},$$

or

$$(a^2+b^2) \zeta + \Omega \frac{(a^2-b^2)^2}{a^2+b^2} = \text{const.} = \tau + \tau'.$$

The surface-integral of vortex-strength over any surface bounded by the principal section in the plane xy is proportional to ζab , so that

$$\zeta ab = \text{const.} = \frac{1}{2} (\tau' - \tau).$$

Hence τ and τ' are both constants.

5. We are now going to suppose that the motion given by (14a) is a small oscillation about the state of steady motion, in which $\Omega=0$ and ζ is a given constant $\zeta = \omega = \sqrt{4\pi\gamma\rho\epsilon}$.

On eliminating σ , the equations (14a) become

$$\left. \begin{aligned} \frac{\tau^2}{(b-a)^3} + \frac{\tau'^2}{(b+a)^3} - \frac{1}{2} \left(\ddot{b} - \frac{\ddot{c}c}{b} \right) &= \frac{1}{2} \frac{\gamma\rho}{b} (Bb^2 - Cc^2) = \frac{\pi\gamma\rho}{b} K \text{ say,} \\ \frac{\tau^2}{(a-b)^3} + \frac{\tau'^2}{(a+b)^3} - \frac{1}{2} \left(\ddot{a} - \frac{\ddot{c}c}{a} \right) &= \frac{1}{2} \frac{\gamma\rho}{a} (Aa^2 - Cc^2) = \frac{\pi\gamma\rho}{a} L \text{ say.} \end{aligned} \right\} \quad (15)$$

* These equations were given by Riemann (*Ges. Werke*, p. 183). They are here deduced by a method similar to that employed by Basset for the general case. See *Proc. Lond. Math. Soc.* vol. xvii. p. 255; or the second volume of his 'Hydrodynamics,' chap. xv.

The motion being oscillatory, we suppose

$$a = a_0 + Qe^{int}, \quad b = b_0 + Re^{int}, \quad c = c_0 + Se^{int}; \quad . \quad (16)$$

a_0, b_0, c_0 being the values of a, b, c in the steady motion. Then the period is $2\pi/n$; and, since the volume is constant, we have

$$Q/a_0 + R/b_0 + S/c_0 = 0. \quad . \quad . \quad . \quad (17)$$

The values of the constants τ^2, τ'^2 are found from (15), by supposing the motion steady, to be

$$\left. \begin{aligned} \tau^2 &= (a_0 - b_0)^{3/4} \gamma \rho \left[\frac{Aa_0^2 - Cc_0^2}{a_0} - \frac{Bb_0^2 - Cc_0^2}{b_0} \right] \\ &= \frac{\pi \gamma \rho}{4} (a_0 - b_0)^3 \left(\frac{L_0}{a_0} - \frac{K_0}{b_0} \right), \\ \tau'^2 &= (a_0 + b_0)^{3/4} \gamma \rho \left[\frac{Aa_0^2 - Cc_0^2}{a_0} + \frac{Bb_0^2 - Cc_0^2}{b_0} \right] \\ &= \frac{\pi \gamma \rho}{4} (a_0 + b_0)^3 \left(\frac{L_0}{a_0} + \frac{K_0}{b_0} \right), \end{aligned} \right\} \quad (18)$$

Now inserting the values (16) for a, b, c in (15) and eliminating S by means of (17), we find

$$\begin{aligned} Q &\left[\frac{3\tau^2}{(a_0 - b_0)^4} - \frac{3\tau'^2}{(a_0 + b_0)^4} - \frac{\pi \gamma \rho}{a_0 b_0} \left(a_0 \frac{\partial K_0}{\partial a_0} - c_0 \frac{\partial K_0}{\partial c_0} \right) + \frac{n^2}{2} \frac{c_0^2}{a_0 b_0} \right] \\ &+ R \left[-\frac{3\tau^2}{(a_0 - b_0)^4} - \frac{3\tau'^2}{(a_0 + b_0)^4} - \frac{\pi \gamma \rho}{b_0^2} \left(b_0 \frac{\partial K_0}{\partial b_0} - c_0 \frac{\partial K_0}{\partial c_0} - K_0 \right) \right. \\ &\quad \left. + \frac{n^2}{2} \left(1 + \frac{c_0^2}{b_0^2} \right) \right] = 0, \quad . \quad . \quad (19) \end{aligned}$$

and

$$\begin{aligned} Q &\left[-\frac{3\tau^2}{(a_0 - b_0)^4} - \frac{3\tau'^2}{(a_0 + b_0)^4} - \frac{\pi \gamma \rho}{a_0^2} \left(a_0 \frac{\partial L_0}{\partial a_0} - c_0 \frac{\partial L_0}{\partial c_0} - L_0 \right) \right. \\ &\quad \left. + \frac{n^2}{2} \left(1 + \frac{c_0^2}{a_0^2} \right) \right] \\ &+ R \left[\frac{3\tau^2}{(a_0 - b_0)^4} - \frac{3\tau'^2}{(a_0 + b_0)^4} - \frac{\pi \gamma \rho}{a_0 b_0} \left(b_0 \frac{\partial L_0}{\partial b_0} - c_0 \frac{\partial L_0}{\partial c_0} \right) \right. \\ &\quad \left. + \frac{n^2}{2} \frac{c_0^2}{a_0 b_0} \right] = 0; \quad . \quad (20) \end{aligned}$$

on eliminating $Q : R$, we obtain the equation for the frequency $n/2\pi$.

6. It is convenient to express the quantities in (19) and (20) in terms of three integrals F, G, H , defined by the

equations

$$\int_0^\infty P_0^{-3} d\psi = F, \quad \int_0^\infty P_0^{-3} \psi d\psi = G, \quad \int_0^\infty P_0^{-3} \psi^2 d\psi = H, \quad (21)$$

which are connected by the identity

$$\frac{2}{a_0 b_0 c_0} = \int_0^\infty \frac{d\psi}{P_0} \left(\frac{1}{a_0^2 + \psi} + \frac{1}{b_0^2 + \psi} + \frac{1}{c_0^2 + \psi} \right), \quad \left. \begin{array}{l} \text{or} \\ 2/a_0 b_0 c_0 = (b_0^2 c_0^2 + c_0^2 a_0^2 + a_0^2 b_0^2) F + 2(a_0^2 + b_0^2 + c_0^2) G + 3H. \end{array} \right\} (22)$$

By differentiating this identity we can find the integrals of the form $\int_0^\infty P_0^{-3} (a_0^2 + \psi)^{-1} d\psi$;

e. g., we have

$$2/a_0^3 b_0 c_0 = (4b_0^2 + 4c_0^2 - 3a_0^2) F + 5G + 3(c_0^2 - a_0^2)(b_0^2 - a_0^2) \int_0^\infty P_0^{-3} (a_0^2 + \psi)^{-1} d\psi. \quad (23)$$

Hence, after reductions, we find

$$\begin{aligned} b_0 \frac{\partial K_0}{\partial b_0} - c_0 \frac{\partial K_0}{\partial c_0} - K_0 &= a_0 b_0 c_0 F [4\{(a_0^2 + c_0^2)b_0^4 + (a_0^2 + b_0^2)c_0^4\} - 3(b_0^6 + c_0^6) \\ &\quad - 3(b_0^2 - c_0^2)^2(a_0^2 - b_0^2 - c_0^2)] \\ &\quad + a_0 b_0 c_0 G [a_0^2(b_0^2 + 3c_0^2) + 2b_0^4 + 2c_0^4 + 6b_0^2 c_0^2] \\ &\quad + a_0 b_0 c_0 H(b_0^2 + 3c_0^2) - 2(b_0^2 + c_0^2); \quad (24) \end{aligned}$$

and

$$\begin{aligned} a_0 \frac{\partial K_0}{\partial a_0} - c_0 \frac{\partial K_0}{\partial c_0} &= a_0 b_0 c_0^3 F (3a_0^2 b_0^2 + a_0^2 c_0^2 + b_0^2 c_0^2) + a_0 b_0 c_0 G (3a_0^2 c_0^2 \\ &\quad + 3b_0^2 c_0^2 - a_0^2 b_0^2 + 2c_0^4) + 2a_0 b_0 c_0^3 H - 2c_0^2. \quad (25) \end{aligned}$$

7. We shall now suppose that the ellipsoid is a spheroid, or that $a_0 = b_0$, and take $a_0^2 = b_0^2 = c_0^2(1 + f^2)$; then we find

$$\begin{aligned} b_0 \frac{\partial K_0}{\partial b_0} - c_0 \frac{\partial K_0}{\partial c_0} - K_0 &= a_0 \frac{\partial L_0}{\partial a_0} - c_0 \frac{\partial L_0}{\partial c_0} - L_0 \\ &= a_0^2 c_0^7 F (10 + 19f^2 + 10f^4 + f^6) + a_0^2 c_0^5 G (14 + 18f^2 + 3f^4) \\ &\quad + a_0^2 c_0^3 H (4 + f^2) - 2c_0^2 (2 + f^2); \quad (26) \end{aligned}$$

and

$$\begin{aligned} a_0 \frac{\partial K_0}{\partial a_0} - c_0 \frac{\partial K_0}{\partial c_0} &= b_0 \frac{\partial L_0}{\partial b_0} - c_0 \frac{\partial L_0}{\partial c_0} \\ &= a_0^2 c_0^7 F (5 + 8f^2 + 3f^4) + a_0^2 c_0^5 G (7 + 4f^2 - f^4) \\ &\quad + 2a_0^2 c_0^3 H - 2c_0^2. \quad (27) \end{aligned}$$

The frequency equation, as found from (19) and (20), gives either

$$n^2 = \frac{2\pi\gamma\rho}{a_0^2} \left[\left(b_0 \frac{\partial K_0}{\partial b_0} - c_0 \frac{\partial K_0}{\partial c_0} - K_0 \right) - \left(a_0 \frac{\partial K_0}{\partial a_0} - c_0 \frac{\partial K_0}{\partial c_0} \right) \right] + \frac{6\tau^2}{(a_0 - b_0)^4} \quad (28)$$

or

$$n^2 \left(1 + \frac{2c_0^2}{a_0^2} \right) = \frac{2\pi\gamma\rho}{a_0^2} \left[\left(b_0 \frac{\partial K_0}{\partial b_0} - c_0 \frac{\partial K_0}{\partial c_0} - K_0 \right) + \left(a_0 \frac{\partial K_0}{\partial a_0} - c_0 \frac{\partial K_0}{\partial c_0} \right) \right] + \frac{6\tau'^2}{(a_0 + b_0)^4} \dots \quad (29)$$

We now introduce the quantity ϵ defined by the equation

$$\omega^2 = 4\pi\gamma\rho\epsilon. \quad (30)$$

The condition of steady motion is

$$f(3 + 2\epsilon f^2) = (3 + f^2) \tan^{-1} f; \quad (31)$$

and we find

$$\tau'^2 = 16a_0^4 \epsilon \pi \gamma \rho. \quad (32)$$

Also by (18) and (32),

$$K_0 = 4a_0^2 \epsilon,$$

or

$$c_0^3 f^2 (a_0^2 G + H) = 2\epsilon. \quad (33)$$

This, with the identity (22), gives G and H in terms of F , viz.,

$$\left. \begin{aligned} (1 + f^2) c_0^5 G &= \frac{2}{f^3(3 + f^2)} \{ f^2 - 3\epsilon(1 + f^2) \} - (1 + f^2) c_0^7 F, \\ c_0^3 H &= \frac{2}{f^2(3 + f^2)} \{ -f^2 + 2\epsilon(3 + 2f^2) \} + (1 + f^2) c_0^7 F; \end{aligned} \right\} \quad (34)$$

where

$$F = \int_0^\infty \frac{d\psi}{(a_0^2 + \psi)^3 (c_0^2 + \psi)^3},$$

so that

$$c_0^7 F = \frac{1}{4f^6} \left[\frac{15 + 25f^2 + 8f^4}{(1 + f^2)^2} - 15 \frac{\tan^{-1} f}{f} \right],$$

or

$$c_0^7 F = \frac{1 + 2f^2}{f^2(3 + f^2)(1 + f^2)^2} - \frac{15\epsilon}{2} \frac{1}{(3 + f^2)f^4}. \quad (35)$$

Also by using (18) and these reductions we find

$$\frac{\tau^2}{(a_0 - b_0)^4} = \frac{\pi\gamma\rho}{2} \left[\frac{2}{3 + f^2} - \frac{\epsilon}{f^2(3 + f^2)} (3 + 8f^2 + f^4) \right]. \quad (36)$$

Equations (28) and (29) now give the two values of n^2 , viz.:

$$n_1^2 = 2\pi\gamma\rho \left[\frac{2}{3+f^2} - \frac{\epsilon}{f^2(3+f^2)} (3+8f^2+f^4) \right] = \frac{4\tau^2}{(a_0-b_0)^4}, \quad (37)$$

and

$$n_2^2 = 4\pi\gamma\rho \frac{1+f^2}{3+f^2} \left[\frac{6}{3+f^2} - \frac{\epsilon}{f^2(3+f^2)} (27+18f^2-f^4) \right]. \quad (38)$$

8. The condition that the expression for n_1^2 in (37) should be positive is Riemann's condition of stability. Using the condition (31) of steady motion, we find that this requires

$$f(3+7f^2) > (3+8f^2+f^4) \tan^{-1} f.$$

The period $\frac{2\pi}{n_2}$ given by (38) reduces to $\pi\sqrt{\frac{5a}{g}}$ in case the spheroid becomes a sphere of the same mean density as the earth, and there is no rotation. This is the period given by Sir William Thomson*.

The period of rotation given by Professor Darwin in his first paper† as most probable for the earth-moon system when the two bodies formed a single mass is 5 hrs. 36 mins.: in a later paper he finds that this is doubtful, but that the period was most likely between two and four hours‡.

I append two Tables, of which I. gives the shorter period $2\pi/n_1$ and the density for different values of f^2 , the period of rotation being 5 hrs. 36 mins.; and II. gives the longer period $2\pi/n_2$, and the density for different values of f^2 and different values of the period of rotation from three to six hours.

TABLE I.

f^2 .	ϵ .	ρ .	T_1 .
0.18	0.02077	5.77	2957 sec.
0.19	0.02177	5.50	3054 sec.
0.20	0.02295	5.22	3177 sec.
0.30	0.03170	3.78	3842 sec.
0.40	0.03969	3.01	4454 sec.

This Table gives the value of the shorter period T_1 and the density when the length of the day is 5 hrs. 36 mins.

* Phil. Trans. 1863.

† Ibid. 1879.

‡ Ibid. 1880.

TABLE II.

ϵ .	f^2 .	$2\pi \cdot \omega$.				
		3 hrs.	4 hrs.	5 hrs.	5 hrs. 36 mins.	6 hrs.
0.02077	0.18	$\left\{ \begin{array}{l} \rho = 5.77 \\ T = 5392 \text{ sec.} \end{array} \right.$	$\rho = 5.04$ $T = 5726 \text{ sec.}$
0.02177	0.19	$\left\{ \begin{array}{l} \rho = 5.50 \\ T = 5443 \text{ sec.} \end{array} \right.$	$\rho = 4.81$ $T = 5850 \text{ sec.}$
0.02295	0.20	$\left\{ \begin{array}{l} \rho = 6.54 \\ T = 5025 \text{ sec.} \end{array} \right.$	$\rho = 5.22$ $T = 5681 \text{ sec.}$	$\rho = 4.56$ $T = 6031 \text{ sec.}$
0.03170	0.30	$\left\{ \begin{array}{l} \rho = 4.73 \\ T = 5751 \text{ sec.} \end{array} \right.$	$\rho = 3.78$ $T = 6549 \text{ sec.}$	$\rho = 3.30$ $T = 6902 \text{ sec.}$
0.03969	0.40	$\left\{ \begin{array}{l} \rho = 5.93 \\ T = 5060 \text{ sec.} \end{array} \right.$	$\rho = 3.78$ $T = 6325 \text{ sec.}$	$\rho = 3.01$ $T = 7164 \text{ sec.}$	$\rho = 2.58$ $T = 7589 \text{ sec.}$
0.04822	$0.52786 = \tan^2 \frac{\pi}{5}$	$\left\{ \begin{array}{l} \rho = 4.88 \\ T = 5487 \text{ sec.} \end{array} \right.$	$\rho = 3.11$ $T = 7019 \text{ sec.}$	$\rho = 2.42$ $T = 7734 \text{ sec.}$	$\rho = 2.17$ $T = 8422 \text{ sec.}$
0.0525	0.60	$\left\{ \begin{array}{l} \rho = 4.48 \\ T = 5531 \text{ sec.} \end{array} \right.$			
0.0578	0.70	$\left\{ \begin{array}{l} \rho = 7.24 \\ T = 4410 \text{ sec.} \end{array} \right.$	$\rho = 4.07$ $T = 6018 \text{ sec.}$			
0.0735	0.80	$\left\{ \begin{array}{l} \rho = 5.47 \\ T = 5814 \text{ sec.} \end{array} \right.$	$\rho = 3.07$ $T = 7752 \text{ sec.}$			

This Table gives the longer period T_2 of oscillation and the density for different values of the length of the day $2\pi/\omega$, and different values of the quantity f^2 on which the ellipticity depends.

XXIX. *The History of the Doctrine of Radiant Energy.*By Lord RAYLEIGH, *Sec. R.S.**

IN his interesting Address † to the American Association for the Advancement of Science, Prof. Langley sketches the development of the modern doctrine of Radiant Energy, and deduces important lessons to be laid to heart by all concerned in physical investigation. This is a most useful undertaking; but in the course of it there occur one or two statements which, in the interest of scientific history, ought not to be allowed to pass without a protest.

After quoting Melloni's very unequivocal conclusion of 1843, that "Light is merely a series of calorific indications sensible to the organs of sight; or, *vice versâ*, the radiations of obscure heat are veritable invisible radiations of light," Prof. Langley goes on to say, "So far as I know, no physicist of eminence reasserted Melloni's principle with equal emphasis till J. W. Draper, in 1872. Only sixteen years ago, or in 1872, it was almost universally believed that there were three different entities in the spectrum, represented by actinic, luminous, and thermal rays."

These words struck me strangely as I first read them. My own scientific ideas were formed between 1860 and 1866, and I certainly never believed in the three entities. Having on a former occasion referred to this question ‡ as an illustration of the difference of opinion which is sometimes to be found between the theoretical and experimental schools of workers, I was sufficiently interested in the matter to look up a few references, with results which are, I think, difficult to reconcile with Prof. Langley's view.

In Young's Lectures§ we read:—"Dr. Herschel's experiments have shown that radiant heat consists of various parts which are differently refrangible, and that, in general, invisible heat is less refrangible than light. This discovery must be allowed to be one of the greatest that have been made since the days of Newton

"It was first observed in Germany by Ritter, and soon afterwards in England by Dr. Wollaston, that the muriate of silver is blackened by invisible rays, which extend beyond the prismatic spectrum, on the violet side. It is therefore probable that these black or invisible rays, the violet, blue, green, perhaps

* Communicated by the Author.

† Amer. Journ. Sci. Jan. 1889.

‡ Address to Section A, Brit. Assoc. Report, 1882.

§ Vol. i. p. 638 (1807).

the yellow, and the red rays of light, and the rays of invisible heat, constitute seven different degrees of the same scale, distinguished from each other into this limited number, not by natural divisions, but by their effects on our senses : and we may also conclude that there is some similar relation between heated and luminous bodies of different kinds."

And, again, on p. 654 : " If heat is not a substance, it must be a quality; and this quality can only be motion. It was Newton's opinion that heat consists in a minute vibratory motion of the particles of bodies, and that this motion is communicated through an apparent vacuum by the undulations of an elastic medium, which is also concerned in the phenomena of light. If the arguments which have been lately advanced in favour of the undulatory theory of light be deemed valid, there will be still stronger reasons for admitting this doctrine respecting heat; and it will only be necessary to suppose the vibrations and undulations principally constituting it to be larger and stronger than those of light, while at the same time the smaller vibrations of light, and even the blackening rays, derived from still more minute vibrations, may perhaps, when sufficiently condensed, concur in producing the effects of heat. These effects, beginning from the blackening rays, which are invisible, are a little more perceptible in the violet, which still possess but a faint power of illumination; the yellow-green afford the most light; the red gives less light, but much more heat; while the still larger and less frequent vibrations, which have no effect upon the sense of sight, may be supposed to give rise to the least refrangible rays, and to constitute invisible heat."

It is doubtless true that Young's views did not at the time of the publication of these lectures* command the authority which now attaches to them. But when the undulatory theory gained acceptance, there was no room left for the distinct entities.

J. B. Reade, one of the pioneers of photography, in a letter to R. Hunt †, of date Feb. 1854, thus speaks of Young :—" Dr. Young's propositions are, that radiant light consists in undulations of the luminiferous æther, that light differs from heat only in the frequency of its undulations, that undulations less frequent than those of light produce heat, and that undulations more frequent than those of light produce chemical and photographic action,—all proved by experiments."

* I may remark, in passing, that Brougham knew a little of experimenting, as of everything else, except law !

† Hunt's ' *Researches on Light*, ' Longmans, 1854, p. 374. Hunt himself, not being an undulationist, was upon the other side.

Sir John Herschel's presentation of the matter * is not very explicit. "The solar rays, then, possess at least three distinct powers: those of heating, illuminating, and effecting chemical combinations or decompositions; and these powers are distributed among the differently refrangible rays in such a manner as to show their complete independence on each other. Later experiments have gone a certain way to add another power to the list—that of exciting magnetism." Although the marginal index runs "Calorific, luminous, and chemical rays," the choice of words in the text, as well as the reference to magnetism (for surely no one believed in a special magnetizing entity), points to the conclusion that Herschel held the modern view.

For the decade between 1850 and 1860, the citation upon which I most rely as indicative of the view held by the highest authorities, and by those capable of judging where the highest authority was to be found, is from Prof. Stokes's celebrated memoir upon Fluorescence †. On p. 465 we read:—"Now according to the Undulatory Theory, the nature of light is defined by two things, its period of vibration, and its state of polarization. To the former corresponds its refrangibility, and, so far as the eye is a judge of colour, its colour." And in a footnote here appended:—

"It has been maintained by some philosophers of the first eminence that light of definite refrangibility might still be compound; and though no longer decomposable by prismatic refraction may still be so by other means. I am not now speaking of compositions and resolutions dependent upon polarization. It has been suggested by advocates of the undulatory theory, that possibly a difference of properties in lights of the same refrangibility might correspond to a difference in the law of vibration, and that lights of given refrangibility may differ in tint, just as musical notes of given pitch differ in quality. Were it not for the strong conviction I felt that light of definite refrangibility is in the strict sense of the word homogeneous, I should probably have been led to look in this direction for an explanation of the remarkable phenomena presented by a solution of sulphate of quinine. It would lead me too far from the subject of the present paper to explain the grounds of this conviction. I will only observe that I have not overlooked the remarkable effect of absorbing media in causing apparent changes of colour in a pure spectrum; but this I believe to be a subjective phenomenon depending upon contrast."

It can scarcely be necessary to insist that "light" is used here in the wider sense, a large part of the memoir dealing with the transformation of invisible into visible light.

The allusion in the note is, of course, to Brewster. This distinguished discoverer never accepted the wave theory, and was thus insensible to the repugnance with which his

* Art. Light, Enc. Met. 1830, § 1147.

† "On a Change of Refrangibility of Light." Phil. Trans. 1852.

doctrine of three different kinds of luminous radiation was regarded by every undulationist. The matter was not finally set at rest until Helmholtz showed that Brewster's effects depended upon errors of experiment not previously recognized.

The following, from W. Thomson*, is almost equally significant :—

“It is assumed in this communication that the undulatory theory of radiant heat and light, according to which light is merely radiant heat, of which the vibrations are performed in periods between certain limits of duration, is true. ‘The chemical rays’ beyond the violet end of the spectrum consist of undulations of which the full vibrations are executed in periods shorter than those of the extreme visible violet light, or than about the eight hundred million millionth of a second. The periods of the vibrations of visible light lie between this point and another, about double as great, corresponding to the extreme visible red light. The vibrations of the obscure radiant heat beyond the red end are executed in longer periods than this ; the longest which has yet been experimentally tested being about the eighty million millionth of a second.”

Again, in Lloyd's ‘Wave Theory of Light’†, we find the following passage :—“It appears, then, that sensibility of the eye is confined within much narrower limits than that of the ear ; the ratio of the times of the extreme vibrations which affect the eye being only that of 1·58 to 1, which is less than the ratio of the times of vibration of a fundamental note and its octave. There is no reason for supposing, however, that the vibrations themselves are confined within these limits. In fact, we know that there are *invisible rays beyond* the two extremities of the spectrum, whose periods of vibration (and lengths of wave) must fall without the limits now stated to belong to the visible rays.”

I believe that it would be not too much to say that during the decade 1850–1860 nearly all the leading workers in physics, with the exception of Brewster, held the modern view of radiation. It would be quite consistent with this that many chemists, photographers, and workers in other branches of science, who trusted to more or less antiquated text-books for their information, should have clung to a belief in the three entities. After 1860, and the discussions respecting the discoveries of Stewart and Kirchhoff, I should have supposed that there were scarcely two opinions. Stewart's ‘Elementary Treatise on Heat’ was published in

* “On the Mechanical Action of Radiant Heat or Light ;” &c. Proc. Roy. Soc. Edinb. Feb. 1852.

† Longmans, 1857, p. 16.

1866, and was widely used in schools and colleges. In book II. ch. II., he elaborately discusses the whole question, summing up in favour of the view that "radiant light and heat are only varieties of the same physical agent, and that when once the spectrum of a luminous object has been obtained, the separation of the different rays from one another is physically complete; so that, if we take any region of the visible spectrum, its illuminating and heating effect are caused by precisely the same rays." What there was further for Draper or any one else to say in 1872 I am at a loss to comprehend*.

To pass on to another point. I have followed the excellent advice to read W. Herschel's original memoirs; but I must confess that the impression produced upon my mind is different in some respects from that expressed by Prof. Langley. It seems to me that Herschel fully established the diversity of radiant heat. In the first memoir† a paragraph is headed "*Radiant Heat is of different Refrangibility*," the question being fully discussed; and from the following memoir (p. 291) it is evident that this proposition extends to invisible radiation. "The four last experiments prove that the maximum of the heating power is vested among the invisible rays; and is probably not less than half an inch beyond the last visible ones, when projected in the manner before mentioned. The same experiments also show that the sun's invisible rays, in their less refrangible state, and considerably beyond the maximum, still exert a heating power fully equal to that of red-coloured light. . . ." Can it then be said of De la Roche that he, in 1811, before anyone else, "derives the just and most important, as well as the then most novel conception, that radiant heat is of different *kinds*"? It was doubtless a most important step when De la Roche and Melloni exhibited the diversity of radiant heat *by means of selective absorption*; but I do not see how we can regard them as the discoverers of the fact.

It would take too long to establish by quotations, but it is pretty evident that in his two earlier papers‡ Herschel leaned to the view that light was not "essentially different from radiant heat." Why then, after laying hands upon the truth, did he let it go, and decide that light and heat are not occasioned by the same rays?

* I have limited myself to citations from English writers, but I have no reason to think that the course of opinion was different in France and Germany.

† Phil. Trans. 1800, p. 255.

‡ See pp. 272, 291, 292.

“The question,* which we are discussing at present, may therefore at once be reduced to this single point. Is the heat which has the refrangibility of the red rays occasioned by the light of these rays? For, should that be the case, as there will be then only one set of rays, one fate only can attend them, in being either transmitted or stopped, according to the power of the glass applied to them. We are now to appeal to our prismatic experiment upon the subject, which is to decide the question.” The issue could not be more plainly stated. The experiment is discussed, and this is the conclusion:—“Here then we have a direct and simple proof, in the case of the red glass, that the rays of light are transmitted, while those of heat are stopped, and that thus they have nothing in common but a certain equal degree of refrangibility. . . .”

I am disposed to think that it was this erroneous conclusion from experiment †, more, perhaps, than preconceived views about caloric, that retarded progress in radiant heat for so many years. We are reminded of Darwin’s saying that a bad observation is more mischievous than unsound theory. It would be interesting to inquire upon what grounds we now reject the plain answer which Herschel thought himself to have received from experiment. I do not recall a modern investigation in which the heat and light absorptions are proved to be equal for the various parts of the visible spectrum. Can it be that after all we have nothing but theory to oppose to Herschel’s facts?

I hope it will be understood that these criticisms, even if they are sound, do not touch the substance of Prof. Langley’s address, which is doubly interesting as coming from one who has done so much himself to enlarge our knowledge of this branch of science.

XXX. *Note on Steel Magnets*. By WILLIAM BROWN, “Thomson Experimental Scholar,” now Demonstrator in Physics, Royal College of Science, Dublin ‡.

NEARLY two years ago I brought before this Society the results of some experiments on the effects of percussion in changing the magnetic moments of steel magnets, which results were subsequently published in the March and May

* Third Memoir, p. 520.

† See Whewell’s ‘History of the Inductive Sciences,’ vol. ii. p. 548 (1847).

‡ Communicated by Sir William Thomson, having been read before the Physical Society of Glasgow University, Oct. 12, 1888.

numbers of this Journal for 1887. The experiments now to be described were carried out in the Physical Laboratory of Glasgow University. The magnets were cylindrical bar-magnets, 10 centim. in length, made from three different specimens of steel of very approximately known composition, and the principal object of this communication is to show the effective lengths of magnets made from the same specimens of steel.

For convenience of reference I here reproduce, in a modified and curtailed form, Tables I., II., III., giving the main results of the papers above mentioned. Table I. gives the relative percentage proportions of all the substances found in the steel, the quantities in specimen I. being taken as unity. Table II. gives the dimensions of the magnets, and Table III. the main features of the collected results.

TABLE I.—Comparative Composition of the Specimens.

Substance.	Number of specimen.		
	I.	II.	III.
Silicon	1·00	0·08	0·17
Manganese	1·00	1·23	3·25
Phosphorus	1·00	1·71	1·55
Sulphur.....	1·00	0·00	0·00
Carbon	1·00	0·25	0·25
Iron	1·00	0·994	0·987

TABLE II.—Dimensions of Magnets.

Number of specimen.	Length of magnet, in centimetres, <i>l</i> .	Diameter of magnet, in centimetres, <i>d</i> .	Dimension, ratio <i>l/d</i> .	Weight of magnet, in grms.
I.	10	0·300	33	5·5
II.	10	0·265	38	4·3
III.	10	0·270	37	4·5

TABLE III.—The magnetic moment per gramme ; the percentage loss due to four falls from a height of 150 centim. ; and the effects of annealing on the different specimens.

No. of the specimen.	Glass-hard.		Annealed one hour at 100° C.		Annealed other two hours at 100° C.	
	Mag. mom.	Per cent. loss.	Mag. mom.	Per cent. loss.	Mag. mom.	Per cent. loss.
I.	60·33	1·37	62·3	2·84	61·42	2·84
II.	72·16	2·85	72·04	3·45	72·60	3·92
III.	70·00	5·25	67·5	4·57	69·42	6·13

No. of the specimen.	Annealed half an hour at 236° C.		Annealed another half hour at 236° C.		Not remagnetized, and left undisturbed for 9 months.	
	Mag. mom.	Per cent. loss.	Mag. mom.	Per cent. loss.	Mag. mom.	Per cent. loss.
I.	62·32	9·04	60·00	15·9	48·9	1·6
II.	68·85	14·42	59·80	29·2	40·7	3·34
III.	65·50	18·61	57·10	26·2	41·5	2·44

The above table is quoted to show the effect of annealing on the different specimens ; it also shows the retentive power of each specimen, which appears to vary inversely as the quantity of manganese in the specimen. Thus specimen III., which has nearly three times as much manganese as either of the other two, in the glass-hard condition has diminished in magnetic moment by 5·25 per cent. ; and specimen II., which has about 20 per cent. more manganese than I., loses 3 per cent. nearly ; whilst the decrease in the magnetic moment of I. is approximately 1·4 per cent. Specimen I., however, as appears from Table I., differs very much from the others in the quantity of silicon it contains, and it alone contains sulphur. By annealing for one hour in oil at 100° C., the magnetic moment of I. has been slightly raised and that of III. lowered, whilst II. remains unaltered, and so on ; a

simple inspection of the table indicating the behaviour of each specimen under the various treatments.

In order to obtain data for the determination of the effective lengths, the correct method would be to take a magnet the length of the longest magnet required, and after tempering, magnetizing, and testing it, to break it successively into a number of lengths and test each part separately after remagnetizing. But in doing so we would assume that every piece of the same specimen would be in the same physical condition. There is also the difficulty in breaking a glass-hard piece of steel so as to have plane ends, and so permit accurate measurement of the length of the magnet.

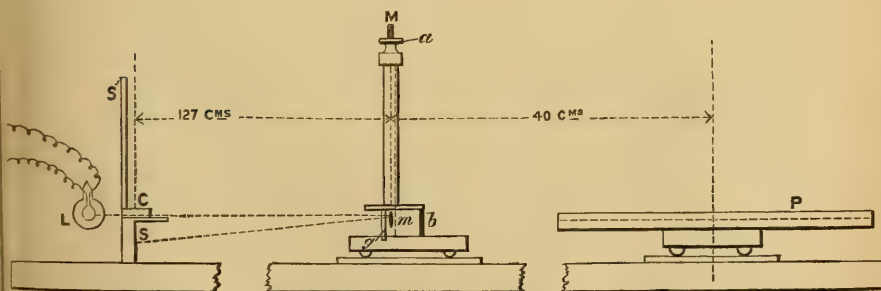
I think, therefore, that there is less liability to error in making all the magnets of the required lengths before tempering, as was done in these experiments.

Each specimen was cut in lengths varying from 1 to 20 centim., they were carefully made straight and the end-planes made as accurately as possible at right angles to their lengths. They were all made glass-hard by heating them to a bright red heat inside an iron tube in a brisk coal fire, and then dropping them end on into a vessel of water 100 centim. deep, the temperature of the water being 7°C . A greater number of magnets than were actually required were treated in this way, and only those which were found to be glass-hard throughout chosen for the experiments.

They were then carefully cleaned, polished, measured, and weighed, and, finally, magnetized to saturation in a long helix giving a field of 1500 C.G.S. units intensity.

In order to obtain the deflexions for calculating the magnetic moments, the apparatus employed consisted of a lamp and scale, a modified form of the Bottomley magnetometer,

Fig. 1.



and a grooved platform for holding the magnets, as shown in fig. 1.

The magnetometer M consists of a small circular mirror m 8 millim. in diameter, with two short magnetic needles 10 millim. long and 0·8 millim. diameter, attached to the back of it and suspended by a single approximately torsionless silk fibre 16 centim. long and $\frac{1}{80}$ of a millim. in diameter; the whole being enclosed in a case of which the base and lower part is wood and the upper part a glass tube 2 centim. in diameter. The upper end of the silk fibre is attached to a pin which can be lowered or raised by means of the nut a working in a collar on a brass cap fixed to the top of the glass tube. This pin is for the purpose of adjusting the height of the mirror so as to allow it to hang centrally in a cavity cut in the wooden block b at the lower end of the tube. The pin is also capable of a lateral motion in any direction by means of three screws (not shown in the drawing) through the brass cap and impinging on the collar. The cavity in the wooden block b for receiving the mirror is 12 millim. in diameter and 3 millim. deep, thus allowing 1 millim. of clearance all round the mirror; the front of the cavity is closed by a piece of thin plate glass g .

The base of the magnetometer is fitted with three conical feet which fit accurately into the well-known hole-slot-and-plane arrangement of Sir William Thomson, the hole and slot being cut in a piece of thick plate glass which is fixed to a table in a position where the horizontal component of the earth's magnetic force is known. Eastwards from the magnetometer at a distance of 127 centim. is a boxwood scale S divided into half-millimetres, and having an electric glow-lamp immediately behind it. The deflexion of the light-spot from the magnetometer-mirror on the scale can be read to $\frac{1}{10}$ of a millim. by means of the shadow cast by a fine wire stretched across the orifice of a copper funnel c through which the beam of light passes from the lamp. A dark screen s serves to prevent any undue reflexion from the lamp on to the scale. Westwards again from the magnetometer, at a distance of 40 centim., is placed the grooved plane P for holding the magnets during the deflexion observations. The base of this plane is made on the same geometrical principle as the magnetometer, and is arranged so that when it is in position a line passing through the middle of the groove and the centre of the magnetometer-mirror shall be at right angles to the magnetic meridian.

The magnet and magnetometer being placed in their respective positions, the magnetic moment per gramme of the magnet was calculated from the well-known formula:

$$M = \frac{H \tan \theta (r^2 - l^2)^2}{2rW};$$

where r = distance in centims. of the centre of the magnet from the centre of the magnetometer-needle,

l = half the distance in centims. between the poles of the deflecting magnet,

$H = 15.1$, the horizontal component of the earth's magnetic force,

θ = the deflexion in degrees of the magnetometer-needle,

W = the weight of the magnet in grammes.

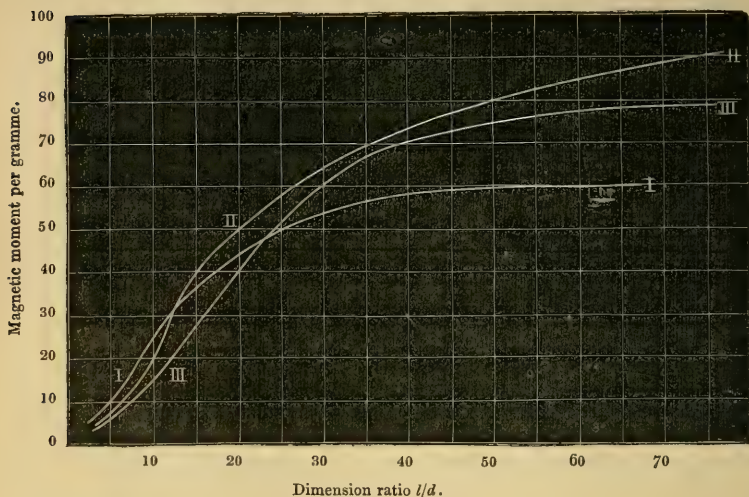
The following Table (IV.) gives the data and calculated magnetic moments of the various magnets made from the three different specimens of steel; and the accompanying curves (fig. 2) show columns 2 and 4 of the table in a graphic form, where the dimension ratio is taken as abscissa and the magnetic moment per gramme as ordinate.

TABLE IV.

Specimen I.			
Length and diameter of magnet, in centims.	Dimension ratio, l/d .	Weight of magnet in grammes.	Magnetic moment per gramme.
20 × .3	66.6	11.0	59.9
15 "	50.0	8.25	59.0
10 "	33.3	5.5	57.0
8 "	26.6	4.4	51.9
6 "	20.0	3.3	44.8
4 "	13.3	2.2	32.5
2 "	6.6	1.1	15.2
1 "	3.3	.55	6.96
Specimen II.			
20 × .265	75.5	8.6	90.5
15 "	56.7	6.4	83.7
10 "	37.7	4.3	71.0
8 "	30.2	3.44	64.6
6 "	22.6	2.58	53.8
4 "	15.1	1.72	41.2
2 "	7.6	.86	12.4
1 "	3.8	.43	5.5
Specimen III.			
20 × .27	74.1	9.0	78.4
15 "	55.6	6.75	75.0
10 "	37.0	4.5	70.0
8 "	29.6	3.6	59.6
6 "	22.2	2.7	45.4
4 "	14.8	1.8	27.3
2 "	7.4	.9	10.7
1 "	3.7	.45	5.0

These curves, when examined along with Table I., are very interesting.

Fig. 2.



From a magnetic point of view, specimen II. seems to be the best of the three; since with a magnet, whose dimension ratio is 19, we get a magnetic moment as great as one with dimension ratio of 23.5 of either specimens I. or III. Specimens II. and III. are about equal with magnets 10 centim. long, or a dimension ratio of 35. We see also that there would be very little gain by making a magnet from specimen I. longer than 8 centim., and 10 centim. appears to be the length for No. III., whilst No. II. rises abruptly up to even 20 centim.

This behaviour of No. II. is somewhat curious; it contains the least silicon and the most phosphorus of the three, and an intermediate quantity of manganese, and in the condition in which it came from the manufacturer it is the hardest and the most elastic of the three specimens; it is also very fibrous, tough, and difficult to straighten.

No. III. is something like II. in its physical qualities, only not so pronounced in the qualities mentioned; whereas No. I. is milder, softer, and more easily worked than the other two. It ought to be mentioned that II. and III. are by one maker, and I. by another.

XXXI. *Notices respecting New Books.*

Journal and Proceedings of the Royal Society of New South Wales.
Vol. XXII. Part I. 8vo, Sydney and London, 1888.

THE Anniversary Address (pages 1–43) by Mr. C. S. Wilkinson, F.G.S., gives a favourable account of the Progress of Science in Australia; and, in noticing his own special subject, the President draws attention not so much to past labours in the field of Geology as to some of the work yet to be accomplished. In sketching out the extent to which each successive series of rock-formations is known, Mr. Wilkinson clearly indicates the points to which further research should be directed. Imperfect or doubtful evidence has to be made good, substantiated, or corrected, as the case may be, both as to age, sequence, and thickness of some strata, and as to the occurrence and characters of fossils. All the good Australian workers in these and other departments of Natural Science are duly mentioned; and in Geology, more particularly the late Rev. W. B. Clarke, the founder of Australian Geology, and the Rev. J. E. Tenison-Woods, to whom the Society's "Clarke Medal" was awarded in May 1888.

In Mr. W. E. Abbott's paper (pages 59–76, with the discussion) on "Forest-Destruction in New South Wales and its effects on the flow of water in watercourses and on the rainfall," the author stated that on his land, a basaltic district, streams now flowed where there was little water when the forests were standing. In the discussion some good points were mooted, as to the decrease of atmospheric pressure, perhaps by solar heat on the exposed ground, allowing the subterranean water to flow out,—as to the local condition of the soil, &c. Although pretty-well established that forests in general do not increase local rainfall, yet the Society quite agreed that, if forests were destroyed, a sufficiency of standing trees should always be left (as, indeed, required by law) for the purposes of shade and ornament.

Mr. H. C. Russell, F.R.S., gives some interesting observations (pages 76–78) on the variable red Star, η Argus. This is now proved to have remarkable fluctuations of brilliance at its minimum as well as at its maximum; and its minimum seems to have now past, as its magnitude is again increasing. Its period is probably about 80 years.

In his "Notes on some Minerals and Mineral Localities in the Northern Districts of New South Wales" (pages 78–88, with a plate), Mr. D. A. Porter continues his remarks (made in 1884) on the same subject, and notices Gold, Antimony, Stibnite, Molybdenite, Actinolite, Axinite, Beryl, Zircon, Spinel, Gahnite, Pleonaste, Vesuvianite, Rhodochrosite, Siderite, Calcite, Arragonite, Natrolite, Heulandite, Chabazite, Analcite, Laumonite, and Stilbite.

Mr. Walter Shellshear, A.M.I., C.E., explains his "Simple plan of easing Railway-Curves" (pages 89–97, with tables and a plate), "without adding (he says) materially to the work of the surveyor, or overtaxing his brain with obtuse formula."

XXXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 206.]

December 19, 1888.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. "*Trigonocrinus*, a new genus of Crinoidea from the 'Weisser Jura' of Bavaria, with description of new species, *T. viratus*; Appendix I. Sudden deviations from normal symmetry in Neocrinoidea; and Appendix II. *Marsupites testudinarius*, Schl., sp." By F. A. Bather, Esq., B.A., F.G.S.

2. "On *Archæocyathus*, Billings, and on other Genera allied thereto, or associated therewith, from the Cambrian Strata of North America, Spain, Sardinia, and Scotland." By Dr. G. J. Hinde, F.G.S.

3. "On the Jersey Brick Clay." By Dr. Andrew Dunlop, F.G.S.

This clay is of a dull yellow colour and somewhat sandy; in places it effervesces with acids; bedding and lamination have been noted. The lower part contains angular stones, usually with their longest diameter parallel to the surface of the underlying rock, and either derived from it or from some other rock not far distant. The bulk of the rocks consists of granite, diorite, rhyolite, quartz-felsite, &c., but there is an argillaceous shale, locally hardened, which is largely developed over considerable areas. The clay occurs in patches, covering all kinds of rocks, and is spread over the raised beaches; it seems more abundant on the higher grounds. A similar clay occurs in Normandy and in the other Channel Islands.

The author was disposed to regard this clay as probably a fluvatile deposit laid down towards the close of the Glacial Period, when the Channel Islands were at a lower level and united to the mainland. Subsequently he conceived that it might be the result of the decomposition of shale, feldspathic porphyry, &c., some sections seeming to show this process as still going on; the clay, too, seems better developed over this class of rock; if so, it would require a moving force more energetic than ordinary rainwash.

January 9, 1889.—H. Woodward, LL.D., F.R.S., Vice-President,
in the Chair.

The following communications were read :—

1. "On the Growth of Crystals in Igneous Rocks after their Consolidation." By Prof. J. W. Judd, F.R.S., F.G.S.

That the characteristic structures of the "granophyric" rock were not acquired by them during the act of consolidation, but have resulted from secondary changes taking place subsequently, was suggested in a former communication to the Society. Addi-

tional evidence was now brought forward concerning the nature of the processes by which these structures—variously known as the micropegmatitic, the centric or ocellar, the pseudospherulitic, the microgranitic, and the drusy or miarolitic—which are found in the peripheral zones and the apophyses of granitic intrusions, must have been produced.

That fragments of crystals in detrital rocks undergo enlargement and redevelopment has been shown by Sorby, Van Hise, Bonney, and many other authors. The fact has also been frequently recognized that curious outgrowths may often be detected in connexion with the crystals of igneous rocks; such outgrowths have usually been regarded, however, as having been formed during the original consolidation of the rock.

In a “labradorite-andesite” (labradorite of French petrographers) belonging to the older or “felstone” series of ejections in the Tertiary volcano of Mull, large crystals of a plagioclase-felspar, near to labradorite in composition, are found to exhibit large and remarkable outgrowths of very irregular forms. The distinction between these outgrowths and the original crystals is rendered very obvious from the circumstance that the original crystals have been corroded by the enveloping magma and contain enclosures of the same, and that they have been much cracked, and sometimes even partially kaolinized before growth recommenced in them. In some cases the crystals have been actually broken and recemented by newly deposited felspar-material.

While there is a general crystallographic continuity between the old felspar-crystals and the new outgrowths from them, the variations in the position of extinction in different portions of the enlarged crystal show that, as growth went on, the composition of successively formed zones gradually and progressively changed from near the Anorthite limit to close upon the Albite limit.

These facts prove that, under suitable conditions, felspar-crystals in solid rock-masses may grow at the expense of the unstable glass-magma by which they are surrounded. This conclusion is in complete harmony with some other recent researches—especially those of Dr. J. Lehmann on the mode of production of the perthite-structure in felspars. In conclusion, the circumstances which have given rise to the exceptionally clear illustration of the processes described in the rock under consideration were explained, and the bearings of the principles enunciated on the theory of metamorphism are indicated.

2. “The Tertiary Volcanoes of the Western Isles of Scotland.” By Prof. J. W. Judd, F.R.S., F.G.S.

In his recently published memoir, “The History of Volcanic Action during the Tertiary Period in the British Isles,” Dr. A. Geikie, while adopting many of the views propounded in a communication made to this Society in 1874, “On the Ancient Volcanoes of the Highlands,” takes exception to certain of the conclusions which are maintained in that paper.

Among the ideas set forth in 1874, of which Dr. Geikie now announces his acceptance, and to which, indeed, he supplies valuable support and confirmation, from his own observations and those of various members of the Geological Survey, are the following :—

(1) The perfect transition between the plutonic rocks of the district (granites and gabbros) and the lavas ("felstones" and basalts), and the dependence of each variety of texture exhibited by them—from the holocrystalline to the vitreous—on the conditions under which solidification took place.

(2) The presence of great masses composed of volcanic agglomerates, breccias and tuffs, with numerous intrusive bosses, sheets, and dykes, at five well-marked eruptive centres, namely Mull, Ardnamurchan, Rum, Skye, and St. Kilda, and the subaerial character of the ejections at these five centres.

(3) The Tertiary age, not only of the lavas, but also of the gabbros and granites found associated with them at these different centres.

The conclusions to which exception is taken are as follows :—

(1) That the ejection of the "felstone" lavas and the intrusion of the granites preceded the appearance of the basalts and gabbros.

(2) That the five centres of eruption mark the sites of as many great volcanic cones, now ruined and dissected by denudation.

The view that the acid rocks were, as a whole, older than the basic ones, was originally put forward by Prof. J. D. Forbes and Dr. F. Zirkel, and is supported in the memoir of 1874. Dr. Geikie admits that around several of the centres indicated basalts may frequently be seen resting on more acid rocks; but the latter he regards as being, in every case, of an intrusive character; he also allows that the tuffs intercalated with the basalts often contain fragments of felsite, but he does not accept this as a proof that the felsites must have been erupted before the basalts. Much of the divergence of opinion that has arisen appears, however, to be due to the circumstance that Dr. Geikie classes as basalt many of the dark-coloured lavas (augite-andesites &c.) which were, in the original paper, grouped under the name of "felstones." In these "felstones" the granites and gabbros alike were shown to be intrusive; and it was also admitted that there were many intrusions of acid rocks of later date than both the "felstones" and the basaltic lavas.

With respect to the existence of great volcanoes in the district, Dr. Geikie, while confirming most of the statements which were made in 1874 as to the several centres of eruption, prefers to refer the origin of the great plateaux of basaltic lava to "fissure-eruptions." He maintains that the numerous basic dykes of the district mark the actual cracks through which the lavas in question rose up and welled out at the surface.

In opposition to this view, it was pointed out that the numbers and dimensions of the Tertiary dykes are not such as would warrant us in inferring that they formed the conduits through which the enormous masses of lava forming the plateaux were erupted; and the absence of all proofs of contact-metamorphism at their sides, and of evidence that the majority of them ever reached the surface at all, was commented upon. In 1874 it was pointed out that some of these

dykes appeared to mark the radial fissures on which sporadic cones ("pays") were thrown up, after the great central volcanoes became extinct; and this view is supported by the circumstance of the close analogies between the materials erupted at this later period, and the rocks which constitute some of the undoubtedly Post-Mesozoic dykes.

Dr. Geikie supports his view, that the plateau-basalts of the Western Isles of Scotland and of Antrim were formed by "fissure-eruptions," by facts which he noticed in the Snake-River country, in the year 1879, while he was making an excursion to the Yellowstone Park, and also by observations made by Captain Dutton in the Grand Cañon country, in Utah, and in New Mexico.

With respect to Dr. Geikie's own observations, it was pointed out that geologists who have had more time and opportunity for the detailed study of the district in question, like Captain Reynolds, Dr. Hayden, and Mr. Clarence King, all agree that there is abundant evidence of ordinary volcanic action having occurred in the Snake-River country; and the last-mentioned author distinctly points out the great paucity of dykes, and the absence of any evidence of the existence of fissures such as those from which "fissure-eruptions" are supposed to have taken place.

Captain Dutton, although originally inclined to refer the lava-fields of the Western Territories of the United States to "fissure-eruptions," has, since his visit to Mauna Loa, and his study of the floods of basalt that have flowed from that volcano, very candidly confessed that, in view of these later observations, he is no longer prepared to maintain his original position.

If the effusive action taking place at many volcanoes be rightly understood and appreciated—and the recent very interesting researches of Prof. J. D. Dana in the Sandwich Islands have thrown much new and important light on this subject—the theory of "fissure-eruption" will be found to be as unnecessary as it is vague. At some volcanic centres there is a preponderance of explosive action; at others the main result consists in the extrusion of lavacurrents; while in most cases we find a combination of both kinds of action. The Tertiary volcanoes of Scotland, like the existing volcanoes of Iceland, are interesting as exhibiting evidence of both the effusive and the explosive action on the very grandest scale.

January 23.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "On the prevailing Misconceptions regarding the Evidence which we ought to expect of former Glacial Periods." By Dr. James Croll, F.R.S., F.G.S.

The imperfection of the geological record is greater than is usually believed. Not only are the records of ancient glacial conditions imperfect, but this follows from the principles of geology. The evidence of glaciation is to be found chiefly on *land-surfaces*, and the ancient land-surfaces have not, as a rule, been preserved.

Practically the several formations consist of old sea-bottoms, formed out of material derived from the degradation of old land-surfaces. The exceptions are trifling, such as the under-layers of coal-seams, and dirt-beds like those at Portland. The transformation of an old land-surface into a sea-bottom will probably obliterate every trace of glaciation; even the stones would be deprived of their ice-markings; the preservation of Boulder-clay, as such, would be exceptional. The absence of large erratic blocks in the stratified beds may indicate a period of extreme glaciation, or one absolutely free from ice. The more complete the glaciation the less probability of the ice-sheet containing any blocks, since the rocks would be covered up. Because there are no large boulders in the strata of Greenland or Spitzbergen, Nordenskjöld maintains that there were no glacial conditions there down to the termination of the Miocene period. The author maintained that glaciation is the normal condition of polar regions, and if these at any time were free from ice, it could only arise from exceptional circumstances, such as a peculiar distribution of land and water. It was extremely improbable that such a state of things could have prevailed during the whole of the long period from the Silurian to the close of the Tertiary.

A million years hence it would be difficult to find any trace of what we now call the glacial epoch; though if the stratified rocks of the Earth's crust consisted of old land-surfaces, instead of old sea-bottoms, traces of many glacial periods might be detected. The present land-surface will be entirely destroyed in order to form the future sea-bottom. It is only those objects which lie in existing sea-bottoms which will remain as monuments of the Post-tertiary glacial epoch. Is it, then, probable that the geologist of the future will find in the rocks formed out of the non-existing sea-bottom more evidence of a glacial epoch during Post-tertiary times than we now do of one, say, during the Miocene, Eocene, or Permian period? Palæontology can afford but little reliable information as to the existence of former glacial periods.

2. "On Remains of Eocene and Mesozoic Chelonia, and on a Tooth of (?) *Ornithopsis*." By R. Lydekker, Esq., B.A., F.G.S.

3. "On the Dentition of *Lepidotus maximus*, Wagn., as indicated by specimens from the Kimeridge Clay of Shotover Hill, near Oxford." By R. Etheridge, Esq., F.R.S., F.G.S., and H. Willett, Esq., F.G.S.

XXXIII. Intelligence and Miscellaneous Articles.

ON IRRECIPROCAL CONDUCTION. BY DR. C. FROMME,
PROFESSOR OF PHYSICS IN THE UNIVERSITY OF GIESSEN*.

IN the August number of the Philosophical Magazine, which has only just now come to my notice, Messrs. Haldane Gee and Holden have published experiments relating to the resistance of a voltameter with platinum electrodes filled with strong sul-

* Communicated by the Author.

phuric acid and polarized by a great electromotive force. It has manifestly escaped the notice of the authors that the phenomena which they have discovered were for the greater part already described by me in the January number of Wiedemann's *Annalen*,* entitled "Ueber das Maximum der galvanischen Polarisation von Platinelectroden in Schwefelsäure." In this research I have communicated experiments on the influence of the concentration of the acid, as well as the influence of the size of the electrodes, on the value of the maximum polarization, and therefore also on the values of the resistance of the voltameter which occur at the same time as the values of the polarization. The subject of the researches of Messrs. Gee and Holden is exclusively the observation of the resistance; and it entirely confirms the results which I published. As regards also the cause of the interesting phenomena, the authors confirm the conclusions which I have made known.

I may therefore here restrict myself to a brief collation of those passages in my paper from which are seen the great variations in the values both of the polarization and of the resistance of a voltameter, which change with the magnitude of the resistance of the rheostat; assuming that the voltameter contains sulphuric acid of 47° to 57° per cent., and between a *small polished* platinum anode and a platinum kathode of any size and condition.

The electromotive force of the polarizing current may amount to 10–12 volts. If, then, the current is closed with a great resistance in the rheostat, innumerable small bubbles of gas arise from the kathode as well as from the anode, the deflexion of the galvanometer is very constant; the resistance of the voltameter is greater than that given by the law of resistance of sulphuric acid, and its polarization somewhat less than if it had been filled with acid of 40 to 45 per cent.

If, now, the resistance of the rheostat is diminished, the strength of the current at first increases, the number of bubbles of gas ascending from the electrodes increases, while their magnitude at first remains small. The needle of the reflecting-galvanometer of a sudden begins to oscillate strongly, and all at once sinks to a considerably smaller deflexion, making continually smaller oscillations about this. While a uniform current of small gas-bubbles continually ascends from the kathode, only larger gas-bubbles now rise from the anode with a hissing noise and at longer intervals, 1 to 3 bubbles in the second according to the strength of the current. This intermittent escape of gas-bubbles from the anode is the cause of the oscillations of the needle, which take place even with a strongly damped galvanometer. In this condition the polarization of the voltameter is 1 to 2 volts higher than in the preceding case; its resistance, however, contrary to the law of resistance of sulphuric acid, is *smaller* than when it was filled with 40 to 45 per cent. sulphuric acid.

We still more diminish the resistance of the interposed rheostat; the current first of all increases. But suddenly the needle flies

* C. Fromme, Wiedemann's *Annalen*, xxxiii. pp. 80–216 (1888).

back and sets continuously very near its position of rest. A small but perfectly uniform stream of minute gas-bubbles still ascends from the kathode, but the anode is covered with a thick bubble of gas, and this only escapes every 10 to 20 seconds. As it escapes, the current momentarily increases a little, and again decreases in proportion as the new bubble forms. In this condition the resistance of the voltameter is of enormous magnitude; but its polarization does not exceed that obtained by introducing a large rheostat-resistance, or by filling the voltameter with 40 to 45 per cent. acid; it is only the resistance, and not the polarization, of the voltameter which is of abnormal magnitude. On the other hand, in what has been described above as the middle condition, both the polarization and the resistance are of abnormal magnitude, the former too large and the latter too small. I will, however, remark that this middle condition is the less easy to obtain, the nearer the concentration of the acid is to 60 per cent. The first condition passes quickly into the third when the resistance of the rheostat is diminished.

PHOTOGRAPHY OF THE SOLAR SPECTRUM.

To the Editors of the Philosophical Magazine and Journal.

Survey of India Offices, Calcutta,
January 7th, 1889.

GENTLEMEN,

With reference to the very interesting article on "Photography of the Least Refrangible Portion of the Solar Spectrum," by J. C. B. Burbank, which appears in your Journal for October last, will you kindly permit me to point out that my early results in photographing the lines on the less refrangible side of A, which the author attributes to the use of turmeric, were for the most part obtained on collodio-bromide plates stained with a *blue* dye, (ordinary aniline blue), the lines in this part of the spectrum being reversed, *i.e.* dark, on a clear ground. I found, however, at the same time that some collodio-bromide plates, stained with the tincture of the seeds of the annatto plant (*Bixa orellana*), not turmeric, were unusually sensitive to the whole spectrum, so that I obtained the spectrum up to A and traces of a line below it unreversed. Apart from the reversing, the best results were secured with the *blue*-stained plates. An account of these experiments was published in the Proceedings of the Royal Society, No. 166 of 1876.

I may take this opportunity of recommending the treatment of cyanin proposed by Messrs. Wellington and Burbank. Plates stained with the dye so treated are exceedingly sensitive in the red, and work much cleaner than if stained with the ordinary cyanin. On Wratten and Wainwright's ordinary plates so treated, I have obtained very good small photos of the spectrum from C to A, but, although there is a considerable extension of action below A, there are only just traces of lines.

I remain, Yours truly,
J. WATERHOUSE, Lieut.-Col. B.S.C.

ON SOME EXPERIMENTS WITH THE SPARK OF A LARGE BATTERY. BY A. RIGHI.

The battery which I have had made and which has given me the best results, consists of 108 condensers. Each of them is a cylindrical beaker more than half a metre high, and 16 centim. in diameter. The coating extends to about half the height, and each has a surface of about 1432 square centims. The glass is rather more than a millim. thick, and, accordingly, the capacity of each beaker is about 6270 electrostatic units (C. G. S.). The arrangement of the conductors of the battery is that described in a memoir on the electric spark, published by myself in 1875.

The 108 jars are arranged in 6 batteries of 18 each arranged in cascade, by which high potentials can be obtained. The terminal coatings are connected with the conductors of a Holtz machine, and the armature of the middle is put to earth. It has thus the same capacity as if $\frac{18}{6} = 3$ jars together were joined as a battery,

with the armatures directly communicating with two conductors of the machine; hence the capacity of the system will be 18,810 electrostatic units (C. G. S.), or about $\frac{1}{48}$ of a microfarad.

The Holtz machine is like one which I have described elsewhere*, but has four disks. It ordinarily gives sparks more than 30 centim. in length; and this even on the most humid days, for it is enclosed in a glass case containing chloride of calcium, together with a small frictional machine to give the initial charge. The disks are unvarnished, and it is sufficient to clean it from time to time with alcohol to obtain the best results.

It happened more than once that the battery discharged through the machine, leaving deep marks on the disks, and one discharge even perforated two of the large sides of the glass case. In order to prevent these evils, and also for the protection of the person who worked the machine, I connected the two conductors and the terminal coatings of the battery by a long glass tube filled with water. With this arrangement, if the discharge took place in the machine it could not do any damage, nor be dangerous, for there is a very high resistance in the circuit. The connexions with the apparatus in which are produced the discharges to be studied are metallic, being made with long wide brass tubes.

This battery, which has served me for experimentally illustrating a special course on atmospheric electricity, gives on a still larger scale the remarkable effects which I have elsewhere described†. For instance, if in the circuit we place two brass spheres 6 to 7 centim. in diameter, at distances of 5 to 10 centim. apart, and place between them a flat strip of glass 5 metres long or more, coated with zinc-filings like the magic pane, we obtain on the strip, instead of the usual luminous ramifications, a loud and

* *Descrizione ed uso di una macchina &c. sull' Acc. di Bologna, 1879.*

† *Loc. cit.*

large spark, 5 metres or longer; and with this experiment we can account in part for the enormous length of lightning-flashes, assuming that the filings represent the minute droplets of water suspended in the atmosphere. Along the surfaces of water (in my case placed in as many large glass troughs arranged in series) we obtain a discharge more than a metre in length, and of equal length if discharged through a large gas-flame.

The following is the new experiment which has given occasion to this Note.

A platinum wire, $3\frac{1}{2}$ metres in length (or even a little longer) and $\frac{1}{20}$ of a millim. in diameter, being fused by the discharge, is instantly changed into a beautiful corona of incandescent globules; but if we take a shorter path in the same wire, for instance a metre and a half, we observe the following curious phenomenon. The moment the discharge takes place, a white spark a metre and a half in length is observed in the place occupied by the wire, which is rectilinear if the wire is straight, but follows the shape of the wire if bent. Of course no trace is observed in the wire behind the spark; there is only produced from this long spark a little smoke with a characteristic odour.

With iron, brass, or gold wire, with a thin and very narrow ribbon of steel, or of magnesium, or tinfoil, an analogous phenomenon is observed. The spark becomes yellow with iron and with gold, and green with copper. With these metals the smoke of the discharge is more dense and abundant, but does not produce the penetrating odour which platinum does.

The formation of this spark may be explained as follows. The first portions of the discharge are sufficient to convert the wire into the state of vapour; the remainder of the discharge then finds a column of metallic vapour at a high temperature which offers an easy path. It forms instantaneously, as it were, a Geissler's tube, the sides of which are formed of the surrounding cold air, full of rarefied gas, because at a high temperature.

To test this explanation, I devised the following experiment. Above about the middle of the wire A B (which is stretched between two stouter ones) I place a conductor with a knob C, which is connected with the stout wire to which A is attached. If the above explanation is correct, this is what should happen. The moment the discharge takes place, this should commence by traversing the wire A B and volatilizing it, provided the knob is at a suitable distance from the wire; but then, instead of forming a spark from A to B through the metallic vapour, it should form one simply from C to B. Then the wire should evaporate altogether, but the large spark appear only on the right of C.

Having frequently made the experiment, I have observed that it succeeds completely as prevised, and that the moment the discharge passes all the wire evaporates, and only a spark passes from C to B. —*Bull. Acad. dei Lincei*, Dec. 16, 1888.

MEASUREMENTS OF RADIATION ON THE SONNBLICK IN
FEBRUARY 1888. BY DR. J. M. PERNTER.

The observations were made with Violle's actinometers, which were modified so as to measure the radiation, and were simultaneously effected on the Sonnblick (3095 metres) and the Rauris (900 metres).

The value of the radiation S_1 in Rauris, at a temperature of -8°C. , was found to be 0.151 calorie; and on the Sonnblick, with a temperature of -15° , it was $S_2 = 0.201$ calorie (gramme minute).

From this the temperature of cosmical space is calculated to be -111°C.

It follows moreover from this that the *entire* atmosphere possesses *unit* power of absorption for rays proceeding from the earth. This, however, is not the case with the atmosphere on high mountains such as the Sonnblick. Hence no rays pass into the cosmical space from the lower levels, that is, from the greater part of the earth's surface, as maintained by Langley; but they do so pass from the tops of the hills.

As, for the future, we are able to calculate the radiation of the atmosphere from an observation of radiation, and the coefficient of radiation for the entire atmosphere is equal to unity, we can determine the mean temperature of the atmosphere from each measurement of radiation, for $\sigma = AT^4$.

From a comparison with measurements of Maurer, in Zurich, it follows that the radiation of the atmosphere only depends on its temperature, and, as a further conclusion, on the temperature observed on the earth's surface.

It can moreover be easily calculated from the above that, with complete absence of the sun, the mean temperature of the earth would be lowered by 103° , that is would amount to -88°C.

By the aid of the results of measurements of the radiation the solar constant may, curiously enough, be calculated with at least the same accuracy as from direct measurements of the solar radiation. It is about 3.1 calories.—*Berichte der Wiener Akademie*, December 1888.

THEORY OF ISOHYDRIC SOLUTIONS. BY SV. ARRHENIUS.

The author has applied the term *isohydric* to two solutions the conductivities of which are not altered when they are mixed (Wied. *Ann.* vol. xxx. p. 51, 1887). According to the author's views, what is called the electrolytic dissociation (into ions) of the two electrolytes is unaltered in these circumstances when they are mixed. Since, according to the investigations of Van't Hoff, the same conditions of equilibrium hold for dilute solutions as for gases, from the principle of entropy we can establish the conditions which two isohydric solutions must obey, so that on mixture their condition of dissociation is not altered. These conditions lead to

the result that isohydric solutions contain equal dissociated parts per unit of volume. The author compares this theoretical conclusion with the results of his determinations of isohydric solutions, and finds a satisfactory agreement in the twenty-two cases which can be calculated. The author deduces further in the same way the general properties of isohydric solutions as found experimentally.

If to a solution of an electrolyte, a second electrolyte is added which has one ion common with the first, the state of equilibrium between the non-dissociated and the dissociated parts (the ions) of the first electrolyte is displaced, and so that the dissociation is less. This is particularly remarkable when the first electrolyte is a feeble acid or base. As moreover, according to the author, the facility with which a body reacts is proportional to the dissociated portion, then from the general conditions of equilibrium the influence of extraneous electrolytes on the velocity of reaction may be calculated. Such a calculation has been made for the case formerly investigated experimentally by the author, the saponification of ethyle acetate by ammonia, and yields results which agree with experiment. The author concludes from this calculation, that all ammonia salts act in this case in the same manner, as has been found. Other regularities observed in saponification may be deduced in the same way.

The author proves in conclusion, that if quantities a , b , c , d of four isohydric solutions of electrolytes, I_1J_1 , I_1J_2 , I_2J_1 , and I_2J_2 , are mixed with each other, there can be no chemical change between these four bodies, provided only $a \cdot d = b \cdot c$. A similar conclusion can be drawn for any given number of electrolytes. The chemical equilibrium between several electrolytes in the same solution may consequently be calculated from these regularities.—*Zeitschrift für Phys. Chem.* p. 284 (1888); *Beiblätter der Physik*, vol. xii. p. 678 (1888).

ON AN ELECTROCHEMICAL ACTINOMETER.

BY MM. GOUY AND RIGOLLOT.

Two copper plates, one of which was heated in a Bunsen's flame until the iridescent colours disappeared and the plate had become of a homogeneous brown, while the other is bright, or both are oxidized, are immersed in solution of sodium chloride. When light falls on an oxidized plate it becomes more strongly positive. The action is instantaneous and disappears in darkness: the apparatus is sensitive to all colours. On closing the circuit by a few ohms resistance the alteration of electromotive force is somewhat greater. Bromides act similarly, iodides somewhat more feebly. When the copper plates are heated too strongly they become less sensitive. They may advantageously be coated on the back with paraffin during cooling.—*Comptes Rendus*, p. 1470 (1888); *Beiblätter der Physik*, vol. xii. p. 681 (1888).

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[FIFTH SERIES.]

APRIL 1889.

XXXIV. *On Rays of Electric Force.*
By Prof. Dr. H. HERTZ*.

AFTER I had succeeded in showing that an electric oscillation could give rise to a wave capable of radiating into space, I at once made the attempt to intensify this effect and to make it sensible at greater distances by placing the exciting conductor in the focus of a large parabolic mirror. These attempts did not succeed, and I convinced myself that the failure was the necessary result of the wrong proportion existing between the length of the waves employed, viz. 4 to 5 metres, and the dimensions of the mirror in the most favourable case possible. I have lately observed that the experiments described by me can quite well be performed with oscillations more than ten times as rapid, and with waves more than ten times as short as those at first discovered. I have therefore returned to the use of concave mirrors, and have attained better results than I had ventured to expect. I have succeeded in producing distinct rays of electric force, and in performing with them the elementary experiments which one is accustomed to perform with light and with radiant heat. An account is here given of these experiments.

The Apparatus.

The method of producing short waves is the same as that by which the longer waves were excited. The primary con-

* Translated from the *Sitzungsb. der Akad. der Wiss. Berlin*, Dec. 13, 1888.

ductor employed may be most simply described as follows:—Imagine a cylindrical brass body, of 3 centim. diameter and 26 centim. length, interrupted in the middle of its length by a spark-space of which the poles on each side consist of spherical surfaces of 2 centim. radius. The length of the conductor is nearly equal to half the wave-length of the corresponding oscillations in straight wires; we can thus form at once an approximate estimate of the period of oscillation. It is essential that the pole-surfaces of the spark-interval should be frequently polished, and during the experiments carefully guarded against illumination by simultaneous lateral discharges, otherwise the oscillations are not obtained.

The aspect and the sound of the spark both give notice whether the spark-interval is in a satisfactory condition. The discharge is conducted to the two halves of the conductor by means of two thick wires covered with gutta-percha, which terminate near the spark-interval on the two sides. As inductorium I found it advantageous to employ, instead of the large Rühmkorff's apparatus, a small coil by Keiser and Schmidt, capable of giving sparks of, at most, 4.5 centim. length. It was worked by three accumulators, and gave between the spherical surfaces of the primary conductor sparks of from 1 to 2 centim. long. The spark-interval was then adjusted for the experiments to a length of about 3 millim.

The evidence of the electric forces in space was obtained here also by means of the small sparks which they produced in a secondary conductor. As before, I made use partly of a movable circle, which had an oscillation-period nearly equal to that of the primary conductor. This had now a diameter of 7.5 centim., and was constructed of copper wire 1 millim. thick. The one end of the wire carried a polished brass ball of some millimetres diameter; the other end was pointed, and was adjusted by means of a fine screw, insulated from the wire, to an extremely small distance from the brass ball. It will easily be understood that we have to do only with little sparks of a few hundredths of a millimetre in length, and a little practice enables one to judge better from the brightness of the sparks than from their length.

The circular conductor gives only a differential effect, and is not suitable for placing in the focal line of a concave mirror; it was therefore combined with another secondary conductor of the following form:—Two straight pieces of wire, 50 centim. long and 5 millim. diameter, were so arranged in the same straight line that the opposing ends were 5 centim. apart. From these ends two wires, 15 centim.

long and 1 millim. thick, led, parallel to each other and at right angles to the first-mentioned wires, to a spark-interval similarly arranged to that of the circular conductor. In this conductor no attention was paid to resonance, which is here not very perceptible. It would have been more simple to place the spark-interval directly in the middle of the straight wire; but the spark-interval could not then have been manipulated and observed in the focus of the concave mirror without the observer obstructing the aperture of the mirror. For this reason the arrangement described was preferred as more advantageous.

The Production of the Ray.

If the primary vibration be now set up in a large free space, the circular conductor in its neighbourhood permits the observation, on a smaller scale, of all the phenomena which I had previously observed and described in the neighbourhood of a larger oscillation*.

The greatest distance at which sparks could be recognized in the secondary conductor was 1.5 metre, or as much as 2 metres with a favourable condition of the primary spark-interval.

The effect is increased on either side if a plane conducting surface is adjusted on the opposite side, parallel to the oscillation and at a suitable distance. If the distance is chosen either very small or somewhat greater than 30 centim. the surface produces a prejudicial effect; it produces a strong reinforcement at distances between 8 and 15 centim., a feeble reinforcement at 45 centim., and is without effect at greater distances. We have previously remarked upon this phenomenon, and we conclude from it that the wave corresponding to the primary oscillation has, in air, a half wave-length of about 30 centim. We might expect a still greater reinforcement by replacing the plane surface by a concave mirror of the form of a parabolic cylinder, in the focus of which the longer axis of the primary oscillation falls. If the mirror is properly to concentrate the action at a distance, it is advantageous to have its focal length as small as possible. But if the direct wave is not immediately to quench the action of the reflected wave, the focal length must not be much less than a quarter of a wave-length. I chose therefore a focal length of $12\frac{1}{2}$ centim., and constructed the mirror by bending a zinc plate, 2 metres long, 2 metres broad, and $\frac{1}{2}$ millim. thick, round a wooden frame of the right curvature into the desired

* H. Hertz, Wiedemann's *Annalen*, xxxiv. pp. 155, 551, 609.

form. The height of the mirror thus became 2 metres, the diameter of its aperture 1·2 metre, and its depth 0·7 metre. The primary oscillation was adjusted in the centre of its focal line. The wires by which the discharge was conducted were allowed to traverse the mirror; the inductorium and the accumulators were placed behind the mirror, and produced no disturbance. If, now, we examine the neighbourhood of the vibration by means of our conductors, we find no action behind the mirror and at one side in general; but in the direction of the optic axis of the mirror the sparks are perceptible to a distance of 5 or 6 metres. By placing a plane conducting surface at right angles to the advancing wave the sparks were perceptible near it to a much greater distance—as much as 9 or 10 metres.

The waves reflected by the conducting surface strengthen the direct waves at certain points. At other points, again, the two waves weaken each other. With the straight conductor we can recognize very distinct maxima and minima in front of the plane conductor, and with the circular conductor the interference phenomena characteristic of stationary waves, which I have previously described. I was able to distinguish four nodes which fell on the wall, at 33, at 65, and at 98 centim. distance from it respectively. Hence we have 33 centim. as a close approximation to the half wave-length of the waves employed, and an oscillation-period of 1·1 thousand millionths of a second, assuming the velocity of light for the velocity of radiation. In wires the oscillation had a wave-length of 29 centim. It seems, then, that also with these short waves the velocity is somewhat less in wires than the velocity in space; but the ratio of the two velocities comes very near to the theoretical value of 1, and does not deviate from it so much as our experiments with longer waves made probable. This remarkable phenomenon still requires explanation. Since these phenomena are manifested especially in the neighbourhood of the optic axis of the mirror, we designate the result as an electric ray issuing from the concave mirror.

I now made a second concave mirror exactly similar to the first, and placed the straight secondary conductor in it, so that the two 50 centim. long wires fell in the focal line, but the two wires leading to the spark-interval traversed the wall of the mirror (being insulated from it) by the shortest route. The spark-interval was thus close behind the mirror, and the observer could adjust and observe it without interrupting the course of the waves. I expected that on intercepting the ray by this arrangement I should be able to recognize it at still greater distances, and I found that I was not mistaken. In

the space at my disposal I could recognize the sparks from one end to the other. The greatest distance to which—by making use of an open door—I could trace the ray was 16 metres; but the results of the experiments on reflexion, to be described immediately, leave no doubt that in open spaces sparks could be obtained up to at least 20 metres. For the remaining experiments so great a distance is not necessary, and it is more convenient if the secondary spark is not too weak; a distance of 6–10 metres is the most advantageous for most experiments. We will now describe the simple experiments which may be made with the ray without difficulty. When the opposite is not expressly remarked, the focal lines of both mirrors are to be supposed vertical.

Rectilinear Radiation.

If a screen of zinc plate 2 metres high and 1 metre broad be placed in the straight line joining the two mirrors at right angles to the direction of the ray, the secondary sparks completely disappear. A screen of tin-foil or of gilt paper gives an equally perfect shadow. A frame placed across the ray leaves the secondary-spark-interval dark whenever it intercepts the ray, and permits the sparks to appear whenever it allows the ray to pass. Insulators do not intercept the ray—it passes through a screen of wood or through a wooden door, and it is not without surprise that we see the sparks produced inside a closed room. If two conducting screens, each 2 metres high and 1 metre broad, are placed diametrically right and left near the ray and at right angles to its direction, they do not influence the secondary sparks at all, so long as the breadth of the slit which they leave between them is not smaller than the aperture of the mirror, that is 1·2 metres. If the slit is made narrower than this, the sparks diminish and disappear when the breadth of the slit becomes less than 0·5 metre. If the breadth of the slit be left 1·2 metres, but the screens be moved sideways out of the direct line joining the two mirrors, the sparks also disappear. If the producing mirror be turned right or left, through about 10° out of the correct position, the sparks become weaker, and when turned through 15° the sparks disappear.

The ray has no geometrically sharp limit or shadow, and it is easy to obtain phenomena due to refraction, but I have not yet succeeded in observing maxima and minima at the edge of a shadow.

Polarization.

The mode of production of our ray leaves no doubt that it

consists in transverse vibrations, and can be "plane-polarized" in the optical sense. This fact, however, may also be shown by experiment. If the receiving mirror be rotated round the ray as axis until its focal line, and consequently also the secondary conductor, have attained the horizontal position, the secondary sparks disappear gradually, and no sparks are to be obtained when the two focal lines are at right angles, even if the two mirrors are made to approach each other closely. The two mirrors behave like the polarizer and analyzer of a polarizing apparatus. I had made an octagonal frame of wood 2 metres high and 2 metres broad, and wound copper of 1 millim. thick wire over this, so that the wires were all parallel and 3 centim. from each other. If now the two mirrors were arranged with parallel focal lines, and the wire screen was moved about in the ray at right angles to its direction so that the wires were at right angles to the plane of the focal lines, the screen produced practically no effect upon the secondary sparks; but if the screen was placed so that its wires were parallel to the plane of the focal lines, the ray disappeared altogether. Thus, in regard to the transmitted energy, the screen behaves to the ray exactly like a tourmaline plate to a plane-polarized ray. The focal line of the receiving mirror was then placed horizontal, in which position, as already mentioned, no sparks were obtained, nor were sparks produced by the introduction of the screen into the ray, so long as its wires were either vertical or horizontal. But if the wire frame was placed in either of the two possible positions so that its wires made an angle of 45° with the horizontal line, the introduction of the screen caused the production of sparks. Evidently the screen produces the resolution of the vibrations into two components, and transmits only that vibration which takes place at right angles to the direction of the wires. This component is inclined at an angle of 45° to the focal line of the second mirror, and when once more resolved by the mirror produces the action upon the secondary conductor. The phenomenon is exactly similar to the brightening produced in the dark field of two crossed nicols by a tourmaline plate introduced in the proper position.

The following remarks upon the subject of polarization may be permitted. With the means employed in the present investigation we are only able to recognize electric force. Its vibrations undoubtedly take place, for a vertical position of the primary vibration, in the vertical plane passing through the ray, and are wanting in the horizontal plane. But from the phenomena we observe with slowly changing currents we

cannot doubt that the electric vibrations are accompanied by vibrations of magnetic force, which take place in the horizontal plane and become zero in the vertical plane. The question in which of the two planes the vibrations of our ray take place does not, then, admit of an answer without data to determine whether the question is of electric or of magnetic vibrations. That the failure to decide an old optical dispute is explained by this consideration was first clearly pointed out by Herr Koláček*.

Reflexion.

We have already proved the reflexion of the wave at a conducting surface by the interference of the reflected and direct waves, and have made use of it in the construction of our concave mirrors. But it is now possible for us to separate the two wave-systems. I placed the two concave mirrors in an open space side by side, so that their apertures faced the same way, and that their axes converged upon a point about 3 metres off. The spark-space of the receiving mirror of course remained dark. Now I placed a vertical plane wall of thin zinc plate 2 metres high and 2 metres broad at the intersection of the axes, so as to be equally inclined to them both. I then obtained a long stream of sparks resulting from the reflected rays. The stream of sparks ceased as soon as the wall was turned about a vertical axis either way through about 15° from the correct position; hence the reflexion is regular, and not diffuse. If the wall was removed from the mirrors, their axes being made to converge always upon the wall, the sparks diminished very slowly. I was still able to recognize sparks when the wall was distant 10 metres from the mirrors; the waves had then to traverse a path of 20 metres. This arrangement might be employed with advantage if it were desired to compare the velocity of radiation in the air with other slower velocities of propagation, *e. g.* that by means of a cord.

In order to obtain the reflexion of the ray at incidences other than normal, I arranged the ray parallel to a wall in which there was a door, and in the neighbouring room to which the door led I placed the receiving mirror so that its optic axis traversed the centre of the door and cut the direction of the ray at right angles. If, now, the plane conducting screen was placed vertically at the point of intersection so as to make angles of 45° both with the ray and with the axes of the receiving mirror, a stream of sparks was produced in the

* Wiedemann's *Annalen*, xxxiv. p. 676.

secondary conductor which also was not interfered with by the closing of the door. If the reflecting screen was turned through about 10° from the right position, the sparks ceased. The reflexion is therefore regular, and the angles of incidence and reflexion are equal. That the action was transmitted from the source to the plane mirror and from there to the secondary conductor could also be shown by interposing shadow-giving screens at different points of this path, when the secondary sparks at once ceased, whilst the screen could be placed anywhere else in the room without effect. With the aid of the circular secondary conductor it is possible to determine the position of the wave-surface in the ray; this was at right angles to the ray, both before and after reflexion, so that in the reflexion it suffers a deviation of 90° .

So far the focal lines of the concave mirrors have been vertical, and the plane of vibration consequently at right angles to the plane of incidence. In order to produce reflexion with the vibrations in the plane of incidence, I adjusted the focal lines of both concave mirrors in the horizontal plane. I observed the same phenomena as in the former case, and was moreover not able to perceive a difference in the intensity of the reflected rays in the two cases. If, on the other hand, the focal line of the one mirror is vertical and that of the other horizontal, no secondary sparks are to be perceived. The inclination of the plane of vibration to the plane of incidence is therefore not altered by reflexion so long as this inclination has one of the values mentioned; but this statement cannot be taken as generally true. It may even be considered as open to doubt whether generally the ray after reflexion is plane-polarized. The interferences which the intersecting systems of waves produce before the mirror and which, as I observed, give rise to characteristic phenomena in the circular conductor, may possibly lead to conclusions in the current problems of the optician as to change of phase and amplitude upon reflexion.

I will mention one other experiment on reflexion by an isotropic surface. The two curved mirrors were again placed side by side as in the experiment on reflexion first described, and opposite to them as a reflecting wall the screen, constructed of parallel copper wires, was placed.

It was seen that the secondary spark-interval remained dark when the wires intersected the direction of the vibrations at right angles, but became brighter as soon as the wires coincided with the direction of vibration. The analogy between our surface conducting in one direction and the tourmaline plate is therefore limited to the transmitted portion of the ray. The

part not transmitted is absorbed by the tourmaline plate but reflected by our surface. If in the last experiment the focal lines of the two mirrors are crossed, we can obtain no sparks in the secondary conductor by reflexion at an isotropic surface; but I convinced myself that the experiment succeeds with reflexion at an anisotropic wire screen, if so placed that its wires are inclined at an angle of 45° to both focal lines. The experiment is easily understood from what has been said.

Refraction.

In order to try if a bending of the ray could be observed upon its passing from air into another insulating medium, I had constructed a large prism of so-called hard pitch, an asphalt-like material. The base was an equilateral triangle 1.2 metre in the side, and with a refracting angle of about 30° . The height of the prism with its refracting edge placed vertical was about 1.5 metre.

But since the prism weighed about 12 cwt. and altogether was inconveniently heavy, it was constructed in three pieces, each about 0.5 metres high, placed one on the other. The mass was cast in wooden boxes, which, since they would have no prejudicial effect, were left round the mass. The prism was placed in a support at such a height that the centre of its refracting edge was at the same height as the primary and secondary spark-intervals. After I had convinced myself that refraction did occur, and had formed an opinion as to its amount, I arranged the experiment as follows:—The producing mirror was placed at a distance of 2.6 metres from the prism, turned towards the one refracting surface, so that the axis of the ray passed through the centre of gravity of the prism and intersected the refracting surface at an angle of 65° . Two conducting-screens were placed near the refracting edge of the prism and near the opposite side, which cut off from the ray every other part than that through the prism. On the side of the emerging ray a circle was traced upon the floor of 2.5 metres radius, with the centre of gravity of the prism as centre. The receiving mirror was moved about upon this, so that its aperture was always directed towards the centre of the circle. If the mirror were placed in the line of the incident ray, no sparks were to be obtained in it; in this direction the prism threw a perfect shadow. Sparks, however, appeared when the mirror was moved towards the second surface of the prism, and first when the angular displacement was about 11° . The sparks increased in intensity up to a deviation of about 22° and then decreased again, becoming imperceptible at about 34° . If the mirror

was adjusted in the position where the strongest effect was observed, and withdrawn from the prism along a radius of the circle, the sparks could be recognized up to a distance of 5 or 6 metres. A screen placed in front of or behind the prism always quenched the sparks; a proof that the action occurred in fact through the prism, and did not reach the secondary conductor by any other part. The experiments were repeated after placing the focal lines horizontal, but without altering the position of the prism. No alteration in the phenomena produced was observed. A refracting angle of 30° and a deviation of 22° in the neighbourhood of minimum deviation corresponds to a refractive index of 1.69. The refractive index for light is given for pitch-like substances between 1.5 and 1.6. The uncertainty of our determination and the impurity of the material employed does not permit of our assigning greater importance to the magnitude or signification of this difference.

We have represented the phenomena investigated by us as rays of electric force. We may in conclusion perhaps regard them as light-rays of very great wave-length. To me at least the experiments described seem eminently fitted to remove all doubt as to the identity of light, radiant heat, and electrodynamic wave-motion. I believe that we shall now with more confidence avail ourselves of the advantages which the assumption of this identity offers, both in the domains of optics and of electricity.

XXXV. *On the Limit to Interference when Light is radiated from Moving Molecules.* By LORD RAYLEIGH*.

IN a recent number of Wiedemann's *Annalen*, Ebert† discusses the application of Doppler's principle to the radiation from the moving molecules of an incandescent gas‡, and arrives at the conclusion that the widths of the spectral lines, as calculated upon the basis of the principle, are much greater than is consistent with experiments upon interference with a large relative retardation. This is a matter of no small importance. Unless the discrepancy can be explained, the dynamical theory of gases would, it appears to me, have received a heavy blow, from which it could with difficulty recover. If it be true that a gas consists of molecules in irregular motion,

* Communicated by the Author.

† Wied. *Ann.* xxxvi. p. 466 (1889).

‡ Lippich, Pogg. *Ann.* cxxxix. p. 465 (1870). Rayleigh, 'Nature,' viii. p. 474 (1873).

and that for the most part each molecule radiates independently, there seems no escape from the conclusion that the character of the aggregate radiation must be governed by Doppler's principle.

If v be the velocity of a molecule, θ the inclination of its motion to the line of sight, the natural wave-frequency N is changed by the motion into n , where

$$n = N \frac{V + v \cos \theta}{V}, \quad (1)$$

and V is the velocity of light. If Λ, λ be the original and altered wave-lengths, so that

$$\Lambda = V/N, \quad \lambda = V/n; \quad (2)$$

then

$$\begin{aligned} \lambda &= \Lambda \frac{V}{V + v \cos \theta} \\ &= \Lambda \left(1 - \frac{v}{V} \cos \theta \right) \text{ approximately, } . . . (3) \end{aligned}$$

when v/V is small.

As a first approximation, Ebert supposes that the velocity v of every molecule is the same. In this case the spectral band, into which what would otherwise be a mathematical line is dilated, has the limiting wave-frequencies

$$N \left(1 + \frac{v}{V} \right), \quad N \left(1 - \frac{v}{V} \right), \quad (4)$$

and between these limits is of uniform brightness. For the number of molecules whose lines of motion lie between θ and $\theta + d\theta$ is proportional to $\sin \theta d\theta$, and this again by (1) is proportional to dn . It is here assumed that the spectrum is formed upon a scale of wave-frequencies; but for the present purpose the range concerned is so small that it becomes a matter of indifference upon what principle the spectrum is disposed.

The typical case of interference arises when two streams of homogeneous light are superposed, which differ in nothing but phase. If δ denote this difference of phase, the vibrations may be represented by

$$\cos \psi + \cos (\psi + \delta),$$

or by

$$2 \cos \frac{1}{2} \delta \cos \left(\psi + \frac{1}{2} \delta \right); \quad (5)$$

and the intensity is

$$I = 4 \cos^2 \frac{1}{2} \delta. \quad (6)$$

If the two streams are obtained by reflexion at the opposite faces of a parallel plate, the circumstances are somewhat more complicated. But the simple theory is applicable even here as a first approximation, which becomes more and more rigorous as the difference of optical quality between the plate and the medium in contact with it is supposed to diminish. If μ be the index of the plate, Δ its thickness,

$$\delta = \pi + \frac{4\pi\mu\Delta}{\lambda} = \pi + \frac{4\pi n\mu\Delta}{V}. \quad . \quad . \quad . \quad (7)$$

If the plate be of air, $\mu=1$. In any case the variation of μ is small compared to that of n ; so that if Δ denote the equivalent thickness of air, we may take

$$I = 4 \sin^2 \frac{2\pi n\Delta}{V}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

a function of n —the frequency, as well as of Δ and V .

If now the light be heterogeneous, we have nothing further to do than to integrate (8) with respect to n , after introduction of a factor i such that $i dn$ represents the illumination corresponding to dn^* . In the present case, where the intensity is supposed to be *uniform* within limits n_1 and n_2 , and to vanish outside them, we have

$$\begin{aligned} \int I dn &= 4i \int_{n_1}^{n_2} \sin^2 (2\pi n\Delta/V) dn \\ &= 2 \int i dn \cdot \left[1 - \frac{\sin 2\pi\Delta(n_2-n_1)/V}{2\pi\Delta(n_2-n_1)/V} \cdot \cos 2\pi\Delta(n_2+n_1)/V \right]. \end{aligned} \quad (9)$$

From this we fall back on (8), if we suppose that (n_2-n_1) is infinitely small, so that

$$\int I dn = 2 \int i dn \cdot [1 - \cos 4\pi n\Delta/V].$$

The difference between (8) and (9) thus depends upon the factor

$$\frac{\sin 2\pi\Delta(n_2-n_1)/V}{2\pi\Delta(n_2-n_1)/V}, \quad . \quad . \quad . \quad . \quad (10)$$

which multiplies the second term of (9). If we introduce the special values of n_1, n_2 from (4), and denote the angle in (10) by α ,

$$\alpha = 2\pi\Delta(n_2-n_1)/V = \frac{4\pi\Delta}{\Lambda} \cdot \frac{v}{V}. \quad . \quad . \quad (11)$$

* It is here assumed that the range included is too small to give rise to sensible chromatic variation.

So long as α is small, the mode of interference is nearly the same as if $v=0$. This will be the case when Δ is sufficiently small, so that at first the bands are absolutely black. As Δ increases, the distinctness of the bands will depend mainly upon the relative brightnesses of the least and most illuminated parts. If we call this ratio h , and denote by a the numerical value of (10), we have

$$h=(1-a)/(1+a), \quad . \quad . \quad . \quad . \quad (12)$$

or

$$a=(1-h)/(1+h). \quad . \quad . \quad . \quad . \quad (13)$$

Now from (10) it appears that when α is equal to π , or to any multiple of π , $a=0$, and the field is absolutely uniform. Between values of α equal to π and 2π , 2π and 3π , and so on, there are revivals of distinctness, the maxima of which occur at values not far removed from $\frac{3}{2}\pi$, $\frac{5}{2}\pi$, &c. Thus, between π and 2π there is to be found a value of a at least equal to $\frac{2}{3\pi}$, corresponding to $h=\frac{2}{3}$ nearly. At this stage the bands should certainly be visible.

In order to estimate at what point the interference-bands would first disappear as Δ increases, we must make some supposition as to the largest value of h indistinguishable in experiment from unity. Under favourable circumstances in other respects we may perhaps assume for this purpose $h=.95$, so that $a=.025$. Since a is small, α is nearly equal to π . We may take approximately $\sin \alpha=.025\pi$, or $\alpha=.975\pi$. In fact, so long as we take h nearly equal to unity, the precise value makes very little difference to the corresponding value of α , and for the purposes of such a discussion as the present we may suppose with sufficient accuracy $\alpha=\pi$. In this case, by (11),

$$\frac{2\Delta}{\Lambda} = \frac{V}{2v}, \quad . \quad . \quad . \quad . \quad . \quad (14)$$

which gives the retardation (2Δ) measured in wave-lengths in the neighbourhood of which the bands would first disappear. This estimate differs widely from that put forward by Ebert. The latter is equivalent to

$$\frac{2\Delta}{\Lambda} = .15 \frac{V}{v}. \quad . \quad . \quad . \quad . \quad (15)$$

According to my calculation the value of α corresponding to (15) would be 54° , a would be $.86$, and h would be $.075$; so that the bands should be hardly distinguishable from those which occur when $\Delta=0$.

For the grounds of his estimate Ebert refers to an earlier paper *, in which, however, the calculation seems to relate to a problem materially different from the present, that, namely, in which the refrangibility of the light is limited to two distinct values (as approximately in the case of the soda lines), instead of being distributed equally over the same range. In this case (9) is replaced by

$$4 \left\{ 1 - \cos \frac{2\pi\Delta(n_2 - n_1)}{V} \cdot \cos \frac{2\pi\Delta(n_1 + n_2)}{V} \right\}; \quad (16)$$

so that, if α have the same form as in (11), and a' denote the numerical value of $\cos \alpha$,

$$h = (1 - a')/(1 + a'), \quad . \quad . \quad . \quad . \quad (17)$$

as before.

According to (16) the field is first uniform when $\alpha = \frac{1}{2}\pi$, instead of π , as from (9). When $\alpha = \pi$, the bands are again black, and as Δ further increases there is a strictly periodic alternation between blackness and absolute disappearance of the bands.

The substitution for a spectral band of uniform brightness of one in which the illumination is all condensed at the edges explains a large part of the discrepancy between (14) and (15); but even in the latter problem (15) seems to be a very small estimate of Δ . According to (15), $\alpha = 54^\circ$, $\cos \alpha = .59$; so that from (17) $h = .26$. Bands of which the darkest parts are of only one quarter of the illumination of the brightest parts could hardly be invisible.

The more nearly correct formula (14) is itself, however, based upon the assumption that all the vibrating molecules move with the same velocity. This is the origin of the law expressed in (9), according to which the bands should reappear at a retardation greater than that of first disappearance. But the real law of the distribution of velocity is that discovered by Maxwell, if there is any truth in the molecular theory†. That such is the case is recognized by Ebert; and he argues that the broadening of the spectral band due to velocities higher than the mean, will entail a further diminution in the maximum retardation consistent with visible interference‡. I proceed to the actual calculation of the maximum retardation on the basis of Maxwell's law.

* Wied. *Ann.* xxxiv. p. 39 (1888).

† It is here assumed that we are dealing with a gas in approximate temperature equilibrium. The case of luminosity under electric discharge may require further consideration.

‡ In the earlier memoir (Wied. *Ann.* xxxiv.) Ebert appears to regard the capability of interference (*Interferenz-fähigkeit*) of a spectral line as

If ξ, η, ζ be the rectangular components of v , the number of molecules whose component velocities lie at any time between ξ and $\xi + d\xi$, η and $\eta + d\eta$, ζ and $\zeta + d\zeta$, will be proportional to

$$e^{-\beta(\xi^2 + \eta^2 + \zeta^2)} d\xi d\eta d\zeta.$$

If ξ be the direction of the line of sight, the component velocities η, ζ are without influence in the present problem. All that we require to know is that the number of molecules for which the component ξ lies between ξ and $\xi + d\xi$ is proportional to

$$e^{-\beta\xi^2} d\xi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

The relation of β to the mean (resultant) velocity v^* is

$$v = \frac{2}{\sqrt{(\pi\beta)}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

If the natural frequency of the waves emitted by the molecules be N , the actual frequency of the waves from a molecule travelling with component velocity ξ is by Doppler's principle

$$n = N \left(1 + \frac{\xi}{V} \right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

Hence by (8) the expression to be investigated, and corresponding to (9), is

$$4 \int_{-\infty}^{+\infty} \sin^2 \frac{2\pi\Delta}{\Lambda} \left(1 + \frac{\xi}{V} \right) \cdot e^{-\beta\xi^2} d\xi. \quad . \quad . \quad . \quad (21)$$

In (21) we have

$$\begin{aligned} 2 \sin^2 \frac{2\pi\Delta}{\Lambda} \left(1 + \frac{\xi}{V} \right) &= 1 - \cos \frac{4\pi\Delta}{\Lambda} \left(1 + \frac{\xi}{V} \right) \\ &= 1 - \cos \frac{4\pi\Delta}{\Lambda} \cos \frac{4\pi\Delta\xi}{\Lambda V} + \sin \frac{4\pi\Delta}{\Lambda} \sin \frac{4\pi\Delta\xi}{\Lambda V}. \end{aligned}$$

The last of the three terms, being of uneven order in ξ ,

dependent upon other causes than the width of the line and the distribution of brightness over it. In this view I cannot agree. "The narrowness of the bright line of light seen in the spectroscope, and the possibility of a large number of (interference) bands, depend upon precisely the same conditions; the one is in truth as much an interference phenomenon as the other" (Enc. Brit., Wave Theory, vol. xxiv. p. 425). It is obvious that nothing could give rise in the spectroscope to a mathematical line of light, but an infinite train of waves of harmonic type and of absolute regularity.

* This must be distinguished from the velocity of mean square, with which the pressure is most directly connected.

vanishes when integrated. The first and second are included under the well-known formula

$$\int_0^\infty e^{-a^2x^2} \cos 2rx \, dx = \frac{\sqrt{\pi}}{2a} e^{-r^2/a^2};$$

and we obtain

$$2\sqrt{\left(\frac{\pi}{\beta}\right)} \cdot \left[1 - \cos \frac{4\pi\Delta}{\Lambda} \cdot \exp\left(-\frac{4\pi^2\Delta^2}{\beta\Lambda^2V^2}\right)\right]. \quad (22)$$

In conformity with previous notation we may write

$$a'' = \exp\left(-\frac{4\pi^2\Delta^2}{\beta\Lambda^2V^2}\right);$$

or, if we introduce the value of β from (19),

$$a'' = \exp\left\{-\pi\left(\frac{\pi\Delta}{\Lambda} \frac{v}{V}\right)^2\right\}. \quad (23)$$

The ratio of the least and greatest brightnesses is then, as before,

$$h = (1 - a'')/(1 + a'') \quad (24)$$

If we now assume as determining the limit of visibility $h = .95$, we find $a'' = .025$, and from (23)

$$\frac{2\Delta}{\Lambda} = .690 \frac{V}{v}. \quad (25)$$

It appears therefore that the maximum admissible retardation is sensibly *greater* than that calculated (14) upon the supposition that all the molecules move with the mean velocity v , and as much as $4\frac{1}{2}$ times greater than that (15) taken by Ebert as the basis of his comparison with observation.

Under these circumstances it would seem that there is no discrepancy remaining to be explained. It is true that the width of spectral lines is not wholly due to movement of the molecules; but it is possible that this is the principal cause of dilatation when the flames are coloured by the spray of very dilute solutions, as in Ebert's use of the method of Gouy*. Again, it is true that interference-bands are often observed under conditions less favourable than is supposed in the above estimate of h . In Michelson's method, however, the bands may be very black at small retardations; and it seems very probable that at higher retardations bands involving even less than five per cent. of the brightness might be visible. The question is one of very great interest, and I hope that Herr Ebert will pursue his investigations until it is thoroughly cleared up.

March 7.

* *Ann. de Chim.* xviii. p. 1 (1879).

XXXVI. *On the Law of Molecular Force.*
By WILLIAM SUTHERLAND, M.A., B.Sc.*

IN the Philosophical Magazine for August 1886, and July and August 1887, I advanced the hypothesis that the law of molecular force is that of the inverse fourth power of the distance, and considered how far its consequences harmonize with the results of the experiments of Thomson and Joule on the expansion of gases through porous plugs, and with the experiments of Amagat and Andrews on the volume occupied by CO_2 at different temperatures and pressures. In the present paper I shall discuss the same law in the light of the recent beautiful discoveries of Eötvös and Robert Schiff in capillarity, and shall show, as the chief result of the inquiry, the following law of the parameter A in the expression $3Am^2/r^4$, which expresses the force between two similar molecules of mass m at distance r apart, the law of their mutual potential being $-\frac{Am^2}{r^3}$.

In compounds containing C, O, and H the molecule may be considered to have a volume, to which each atom of H contributes an amount very small in comparison with that contributed by an atom of O and of C; while an atom of O, when singly bound to another atom, contributes an amount equal to that of two carbon atoms, and when doubly bound equal to that of three carbon atoms. The volume of such a molecule can then be expressed in terms of that of a carbon atom, and the parameter A varies inversely as the surface of the molecule.

By the volume of a molecule I do not mean what is usually called the molecular volume (an objectionable term, which I would propose to replace by the term molecular domain), but the actual volume of the molecule. Another result of the investigation will be to show that the rate of change of the translatory kinetic energy of nearly all liquid molecules with temperature is the same when measured at low constant pressure.

Eötvös (Wiedemann, xxvii.) announces the following remarkable law:—The rate of variation with temperature of the product of the surface-tension of a liquid by its molecular domain raised to the power two thirds, is the same for all liquids; or, in symbols, if α denote surface-tension, and v molecular domain,

$$\frac{d}{dt}(\alpha v^{\frac{2}{3}}) = \cdot 227.$$

* Communicated by the Author.

He found this to hold for a large number of liquids ; and also that water, the alcohols, and the fatty acids are exceptional at the lower ranges of temperature. The theoretical grounds on which he founds the law are, in the present state of physics, somewhat transcendental ; but as they illustrate the power of the idea of "correspondence," which Van der Waals has developed so brilliantly, they can be reproduced here with advantage.

Let v be the domain of a molecule when it is part of a mass of liquid, and u when part of a mass of gas at the same temperature as the liquid ; then, if the temperatures T_1 and T_2 for two substances are chosen so that $v_1/u_1 = v_2/u_2$, the conditions of the molecules are considered by Eötvös to "correspond." This equation may be written

$$v_1 p_1 / T_1 = v_2 p_2 / T_2 ; \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where p_1 and p_2 are the pressures of saturation at the temperatures T_1 and T_2 , if we consider the vapours of the liquids as approximately perfect gases. Then, according to the method of "correspondence," it is stated that the forces acting on the two molecules and the energies of the two molecules correspond. Consider a plane area over which n molecules are distributed. The pressure on it is $np_1 v_1^{\frac{2}{3}}$; the tension across a line in the surface-film on which l molecules lies is $lv_1^{\frac{1}{3}} \alpha_1$, where α_1 is the surface-tension or surface-energy. Then, from the assumption as to complete correspondence in the states of the two molecules, we have

$$\frac{lv_1^{\frac{1}{3}} \alpha_1}{np_1 v_1^{\frac{2}{3}}} = \frac{lv_2^{\frac{1}{3}} \alpha_2}{np_2 v_2^{\frac{2}{3}}}, \quad \text{or} \quad \frac{\alpha_1}{p_1 v_1^{\frac{1}{3}}} = \frac{\alpha_2}{p_2 v_2^{\frac{1}{3}}}. \quad . \quad . \quad . \quad . \quad (2)$$

Arguing in a similar manner as to correspondence in the change of energy of the two molecules to their surface-energy when they evaporate with absorption of latent heats $m_1 L_1$, $m_2 L_2$ respectively, m_1 and m_2 being the molecular weights, Eötvös writes

$$\frac{m_1 L_1}{\alpha_1 v_1^{\frac{2}{3}}} = \frac{m_2 L_2}{\alpha_2 v_2^{\frac{2}{3}}}. \quad . \quad . \quad . \quad . \quad (3)$$

This last result is known as Waterston's law (Phil. Mag. 1857, vol. xiv.), announced by him without reference to the idea of correspondence ; but, according to Eötvös, the quantities L_1 , α_1 , v_1 , L_2 , &c. must be taken only for corresponding states of the two bodies. We shall see afterwards that Waterston's law cannot be regarded as an accurate physical law when tested by the available experimental material ; but that, as a rough statement of a connexion between surface-tension and latent heat, it can be deduced from the law of the inverse fourth power.

his rich experimental material, published in the *Annalen der Chemie*, vol. ccxxiii., and in Wiedemann's *Beiblätter*, vol. ix. (from the *Mem. della R. Acc. d. Lincei*, 1884), combined with Eötvös' law, will be used to determine the parameter A ; it is therefore necessary briefly to take note of a discussion which has been raised over a certain assumption of Schiff's. He assumes that the angle of contact of liquid with glass is zero in every case in his determinations; but at the same time, in order to take account of the weight of the meniscus in his determination of surface-tension, he gives measurements of the distance between the top and the bottom of the meniscus, as this distance is in many cases different from the radius of the tube. The meniscus surface would appear to be different from the hemisphere, which it ought very approximately to be if the contact-angle were zero. Volkmann pointed out this (*Annalen der Chemie*, vol. ccxxviii.), and recalculated the capillary constants from Schiff's determinations on the assumption that the data for the height of meniscus might be used for determining the angle of contact which he introduces into the calculation for the surface-tension. These recalculations, while they take away from the definiteness of some of Schiff's minor results, do not affect the truth of his broad discovery. There is no doubt that, on theoretical grounds, Volkmann's criticism is sound enough; that the experimental data, as furnished, bore evidence to the existence of a finite contact-angle not taken into account in the calculations. But it must be remembered that the measurements of the heights of the meniscus were undertaken only for use in a small correction; and that such measurements are not competent to give satisfactory evidence of a finite contact-angle, and are certainly not competent to give a measure of it. The manner in which the apparent height of the meniscus varies in Schiff's data with temperature shows in a practical manner the unreliability of such measurements for testing the difficult question of contact-angle. Schiff admits the theoretical justness of Volkmann's criticism as to contact-angle, but appeals to the regularity of his own results as his practical justification. As Volkmann himself has shown (and Quincke also) that the contact-angle for a large number of watery solutions of inorganic salts is zero, and as ordinary meniscus measurements would not demonstrate the fact, we are on the whole safe in assuming with Schiff that all his liquids wet glass (that is, that their contact-angle is zero), until the point is settled by a satisfactory experimental method.

In applying the law of the inverse fourth power to the capillary theory we cannot use directly the formulæ already

developed in Laplace's theory, seeing that there are assumptions involved in them which are not required or permissible with the law of the inverse fourth power.

For the purposes of this paper it will suffice to consider the surface-tension in a meniscus in a circular tube of so small diameter that its surface may be regarded as portion of a sphere, not necessarily a hemisphere, as we must provide for the case where the contact-angle is not zero.

We desire to find the force exerted by the meniscus $APBCPD$ (fig. 1) on a column

of water, PQ , of small section lying along the axis of the tube. The meniscus consists of two parts: one, the surface layer, of variable density, which is enclosed between the two surfaces APB and $A'P'B'$; let the mean density in this layer be $\bar{\rho}$, while ρ is the density in the body of the fluid. The other part of the meniscus $A'ED$, $B'FC$ is therefore of density ρ ; but we

may regard the whole meniscus as of density $\bar{\rho}$, seeing that the parts $A'ED$ and $B'FC$ will contribute a small part of the whole force. In the same way we can regard the column PQ as throughout of density $\bar{\rho}$, seeing that the force exerted on $P'Q$ is a small part of the whole. Now, for purposes of integration, we wish to replace the discontinuous distribution of molecules in the meniscus and column by an equivalent uniform distribution of matter. Accordingly, as I pointed out in a previous paper, we must be careful to provide a certain distance between our continuous meniscus and the base of the continuous column. This distance is not necessarily the same as the mean diameter of the molecular domain, but is a quantity of the same order of magnitude. The problem then

reduces itself to the determination of the resultant attraction of a meniscus $APBCPD$ (fig. 2), the centre of whose surface is at O , on a column QT , of sectional area a , and with its base at Q at a distance h from P . If the molecular force between two molecules of the liquid is $3Am^2/r^4$, then, expressing the

Fig. 1.

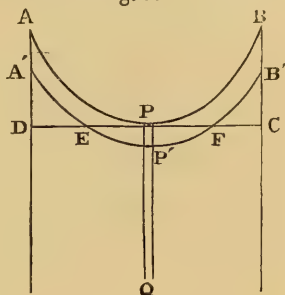
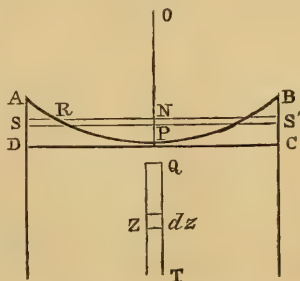


Fig. 2.



force parallel to OT exerted by a ring of radius r about N, and of thickness dl on an element dz of the rod, we get, if $ON=l$ and $OZ=z$,

$$3Aa\bar{\rho}^2 \frac{dz dl 2\pi r dr}{\{r^2 + (z-l)^2\}^{\frac{3}{2}}} \frac{z-l}{\{r^2 + (z-l)^2\}^{\frac{1}{2}}}.$$

Integrate with respect to r from NR to NS=C the radius of the tube, with respect to z from OQ to a limit which we may indicate by the symbol ∞ , and with respect to l from the limit h , which is the distance O from the plane AB up to R, which is OP the radius of the surface, and we have the desired expression :

$$\int_h^R \int_{R+b}^{\infty} \int_{\sqrt{R^2-l^2}}^{\sqrt{R^2-c^2}} 2\pi 3Aa\bar{\rho}^2 \frac{(z-l) dz dl r dr}{\{r^2 + (z-l)^2\}^{\frac{5}{2}}}.$$

The integral evaluated becomes

$$2\pi A\bar{\rho}^2 a \left\{ \frac{(R^2 + (R+b)^2 - 2(R+b)h)^{\frac{1}{2}} - b}{R+b} + \log \frac{b + \sqrt{c^2 + b^2}}{R+b-h + \sqrt{c^2 + (R+b-h)^2}} \right\}.$$

Expanded in powers of b as far as 1st, and with s put for $R-h$, this becomes

$$2\pi Aa\bar{\rho}^2 \left[\log \frac{c}{s + \sqrt{2sR}} + e \left\{ \frac{1}{c} - \frac{1}{R} + \frac{\sqrt{2sR} + R - s - \frac{1}{2}s\sqrt{\frac{2s}{R}}}{R(s + \sqrt{2sR})} \right\} \right].$$

Let us call the angle of contact of meniscus-surface with tube θ . We know, from the usual theory of capillarity verified by experiment, that θ is constant; and this result holds in the same manner for the law of the inverse fourth power, so that, in the above expression, c/R , which = $\cos \theta$, s/R , which = $1 - \sin \theta$, are both constant; and therefore the first term is independent of the curvature of the meniscus, while the coefficient of e may be written

$$2\pi Aa\bar{\rho}^2 \left\{ \frac{1}{R \cos \theta} - \frac{1}{R} + \frac{\sqrt{2(1 - \sin \theta)} + \sin \theta - \frac{1}{\sqrt{2}}(1 - \sin \theta)^{\frac{3}{2}}}{R \{1 - \sin \theta + \sqrt{2(1 - \sin \theta)}\}} \right\}$$

We leave out of count the constant term as not entering into the question of capillary action, and compare our last expression with the expression which we get for the capillary pressure on the column QT, due to a tension α per unit width

of the surface-layer, namely $2Ta/R$. We see that this tension, or the energy per unit surface, is

$$\alpha = \pi \bar{\rho}^2 A e \left\{ \frac{1}{\cos \theta} - 1 + \frac{\sqrt{2(1 - \sin \theta)} + \sin \theta - \frac{1}{\sqrt{2}}(1 - \sin \theta)^{\frac{3}{2}}}{1 - \sin \theta + \sqrt{2(1 - \sin \theta)}} \right\}.$$

This equation is founded on the assumption that the capillary tube is so small that the meniscus-surface may be considered to be a portion of a sphere. When this is not close enough to the truth we shall require a more extended investigation of the necessary form of the meniscus, but for present purposes the above will suffice. It gives us an important relation between the parameter A and the surface-tension, but one involving the unknown average density $\bar{\rho}$ of the surface-film, and the angle θ which is difficult to determine. But, however, as θ , if not zero for all ordinary liquids, is small, we will assume it to be zero, so that the bracketed expression above

reduces to $\frac{1}{2 + \sqrt{2}}$. And as in the first case we desire only

relative values of A for a number of substances, we cannot go far wrong in assuming that, at their boiling-points, the ratios of the mean density of the surface-film of different liquids to the ordinary densities are approximately the same; we will therefore write $\bar{\rho} \propto \rho$, and reduce our equation above to the form $\alpha \propto A \rho^2 e$.

Now it is natural to suppose that, in different liquids, e is proportional to the mean distance apart of the molecules in the surface-film, that is to the cube root of the molecular domain (usually called the molecular volume);

$$\therefore e \propto \sqrt[3]{m/\rho} \propto \sqrt[3]{m/\rho};$$

so that finally $k A \rho^{\frac{5}{3}} m^{\frac{1}{3}} = \alpha$, where k is a constant approximately the same for all liquids if the tension α is measured at the boiling-point; but the strict equation for any one substance is

$$A \bar{\rho}^{\frac{5}{3}} m^{\frac{1}{3}} \propto \alpha.$$

Eötvös's result is that

$$\frac{d}{dt} \left\{ \alpha \left(\frac{m}{\rho} \right)^{\frac{2}{3}} \right\} = .227$$

for most liquids, which, according to our equation above, gives

$$\frac{d}{dt} (k A m \rho) = .227$$

or

$$k A m \frac{d\rho}{dt} = .227$$

This equation contains two important consequences :—

First. The parameter of molecular force varies inversely as the product of molecular weight and modulus of dilatation. [Mendelejeff calls dp/dt the modulus of dilatation, and shows that, for a given liquid, it is approximately independent of temperature when measured at low pressures (one atmosphere).]

Second. The rate of variation of the translatory kinetic energy of the molecules of most liquids with temperature is the same, if that rate is measured at a constant low pressure, such as that of one atmosphere. This second consequence flows from the above equation in the following manner. It is shown in my previous paper (Phil. Mag. July 1887) that the virial of the mutual attractions of the molecules in unit mass of a body, according to the law of the inverse fourth power, is $\pi A \rho \log L/a$; where L is a sensible length such as the cube root of the volume of the unit mass, and a is a length approximately proportional to the cube root of the molecular domain (usually called molecular volume). L/a is so large a number that $\log L/a$ may be regarded as constant within the limits of present experimental possibility in the variation of L and a .

Now Clausius' equation of the virial applied to gases is

$$\frac{3}{2}pv + \text{internal virial} = \text{translatory kinetic energy.}$$

In the case of liquids under a small constant pressure, pv is negligible in comparison with the other terms of the equation; so that for liquids at low pressures the equation assumes the simple form

$$\text{internal virial} = \text{translatory kinetic energy};$$

while for gases at low pressures, the internal virial being negligible, it assumes the form

$$\frac{3}{2}pv \text{ or external virial} = \text{translatory kinetic energy.}$$

In the equation for liquids let us replace the internal virial by its value $3\pi A \rho \log L/a$, and denote the kinetic energy of translation of the molecules in unit mass by E ; then

$$3\pi A \rho \log L/a = E,$$

$$\therefore 3\pi A m \log \frac{L}{a} \frac{dp}{dt} = \frac{d(mE)}{dt}.$$

But $A m \frac{dp}{dt}$ is the same for most liquids,

$$\therefore \frac{d(mE)}{dt},$$

or the temperature rate of variation of the translatory kinetic energy of a molecule, is the same for all molecules when compared as the constituents of liquids at low constant pressure.

This is an important result in molecular dynamics; its similarity to the law of Dulong and Petit as to the constancy of the molecular heats of the elements is obvious and immediately suggestive, while its bearing on the physical meaning of temperature is worth noting. It must not be confused with the assumption made by many continental writers on thermodynamics and molecular physics, that the absolute temperature of a mass is a measure of the kinetic energy of translation of each molecule in the mass, no matter what the state of the mass be; this assumption gives $mE \propto T$, and therefore $d(mE)/dt$ is constant for all substances and for all states of each substance. But the result given above holds only when the external virial is negligible in comparison with the internal; in fact the difference between the two views may be shown thus:—According to the arguments of this paper, we can write $d(mE)/dt = P$ when the external virial is negligible, while $d(mE)/dt = Q$ when the internal virial is negligible, P and Q having possibly different values; while, according to the assumption mentioned above, P is considered equal to Q . Again integrating, we get

$$mE = \int P dt + M \text{ for the first case,}$$

and

$$mE = \int Q dt + N \text{ for the second case,}$$

the constants of integration being possibly different; while, according to the above assumption, the two constants are assumed to be the same, seeing that the translatory kinetic energy of a molecule, both of a liquid and of a vapour at the same temperature, is assumed to be the same and to be proportional to the absolute temperature.

Before proceeding to the determination of the values of A from Schiff's experimental results, it may be as well to discuss a certain old-standing difficulty in capillarity, namely the rapid rate at which capillary height and surface-tension diminish with rising temperature. According to the expression found above, the tension for a given substance varies as the $\frac{2}{3}$ power of the average density of the film, $\alpha \propto \bar{\rho}^{\frac{2}{3}}$. Now C. Schall has shown (*Berichte der deutsch. chem. Gesell.* xiv. p. 555) that approximately the surface-tension for several liquids varies as the $\frac{2}{3}$ power of the density; although the theoretical arguments by which he endeavours to establish this relation as a consequence of the Newtonian law are not

valid, still $\alpha \propto \rho^{\frac{2}{3}}$. The seeming discordance between these two values for α corresponds to the old difficulty in the early days of capillary theory, when Laplace's simplification of imagining the surface-density to be the same as the body-density was adopted, and the surface-tension was found experimentally to diminish much more rapidly with temperature than his theory indicated. But if we take account of the possibility of a markedly different density in the surface from that in the body, and recognize the corresponding difference of stress in the two regions, we shall be prepared to imagine that, approximately, $\bar{\rho}^{\frac{2}{3}} \propto \rho^{\frac{2}{3}}$, which means that the rate of variation of the surface-density with temperature is more rapid than that of body-density. And this is a necessary corollary of our concession of a marked difference of density and stress in the two regions; for Van der Waals has accustomed us to the idea of a difference of stress, of the order of magnitude of 1000 atmospheres; and the recent researches of Amagat, on compressibility at higher pressures and its variation with temperature, have shown that the coefficient of expansion of æther under one atmosphere between 0° and 50° is .0017, while under 3000 atmospheres it is .00056, so that an increase in stress of 3000 atmospheres diminishes the coefficient to one third of its ordinary value. Thus then we see that, in a general way, the old difficulty is really a very direct confirmation of the great difference of stress and density in the surface-film and body of a liquid; and moreover experiment shows that heating the film alone produces a much more pronounced effect than heating the lower liquid only in a capillary tube.

We can now proceed to determine the values of the parameters kA for a large number of compounds by applying the equation $\alpha = kA\rho^{\frac{2}{3}}m^{\frac{1}{3}}$ to Schiff's results.

The following table is devoted to the consideration in the first case of the values of kA for the isomeric forms of the compound ethers of the general formula $C_nH_{2n}O_2$. For the sake of brevity the basic radicals methyl, ethyl, &c. are denoted by I, II, &c., and the basic radicals in ascending order by 1, 2, 3, &c.; so that I 1 stands for methyl formiate, II 3 for ethyl propionate, and so on. Schiff's values of α are given in terms of grammes weight per linear metre, so that kA in the following tables is given in corresponding units. The acids $C_nH_{2n}O_2$ are omitted from the table because Eötvös finds that they do not obey his law, and their anomalous vapour-densities are well known.

Values of $kA \times 1000$.

	$C_3H_6O_2$		$C_4H_8O_2$		$C_5H_{10}O_2$
II 1.....	591	III 1	579	Iso IV 1.....	524
I 2.....	589	II 2	542	III 2.....	501
		I 3	538	II 3.....	497
				I 4.....	494
				I iso 4 ...	487
Mean	589	540	495

	$C_6H_{12}O_2$		$C_7H_{14}O_2$
Iso V 1.....	502	Iso V 2	442
Iso VI 2.....	470	Iso IV 3	423
III 3.....	462	III 4	434
II 4.....	460	III iso 4.....	425
II iso 4	453	II 5	446
I 5.....	460		
Mean.....	461	434

	$C_8H_{18}O_2$		C_8H_{10}		C_9H_{12}
Iso V 3.....	409	Xylol 1	570	Propylbenzol...	521
Iso IV 4.....	397	Xylol 2	566	Ethyltoluol ...	518
Iso IV iso 4 ...	397	Xylol 3	590	Mesitylene.....	506
III 5.....	407	Ethylbenzol ...	570		
Mean	402	574	515

The mean values are taken with the formiates excluded because they depart so markedly from the rest, the formiates evidently tending towards the irregularity of the acids. The mean values are introduced into the following table, in which the values of kA are arranged according to descending order. The second column contains the formulæ with O' to indicate O connected with two other atoms, O'' to indicate O connected with a single atom. Inspection of the table shows that the number of H atoms in the molecule exercises no appreciable influence on the value of kA , and that O' produces the same effect as 2 C, and O'' as 3 C. In the third column there is placed the number n of C atoms that the molecule is equivalent to, which I shall call its *parameter equivalent*. In the fifth column the mean value of kA for a given value of n , or \overline{kA} ; and the last column contains the values of $\overline{kA} n^{\frac{2}{3}}$.

Substance.	Formula.	<i>n.</i>	1000 <i>kA.</i>	1000 <i>kA.</i>	100 <i>kAn</i> ³ .
Methyl cyanide.....	CH ₃ CN	...	1094		
Allylamine.....	NC ₃ H ₅ H ₂	...	950		
Propylamine	NC ₃ H ₇ H ₂	...	940		
Ethyl cyanide	C ₂ H ₅ CN	...	864		
Propyl cyanide	C ₃ H ₇ CN	...	817		
Acetone	C ₃ H ₆ O''	...	806		
Butylamine	NC ₄ H ₉ H ₂	...	799		
Amylene.....	C ₅ H ₁₀	5	794	794	232
Diethylamine.....	N(C ₂ H ₅) ₂ H	...	780		
Butyl cyanide	C ₄ H ₉ CN	...	755		
Amylamine	NC ₅ H ₁₁ H ₂	...	716		
Hexane	C ₆ H ₁₄	6	707		
Diallyl	C ₆ H ₁₀	6	706		
Benzene	C ₆ H ₆	6	705	700	231
Pyridine.....	NC ₅ H ₅	...	703		
Ethyl oxide	C ₄ H ₁₀ O'	6	683		
Aniline	NC ₆ H ₅ H ₂	...	661		
Methyl sulphocyanate	CH ₃ CNS	...	647		
Ethyl sulphide	C ₄ H ₁₀ S	...	634		
Methyl nitrite	CH ₃ NO ₂	...	632		
Toluene	C ₇ H ₈	7	620	620	227
Phenyl cyanide	C ₆ H ₅ CN	...	601		
Triethylamine	N(C ₂ H ₅) ₃	...	600		
Ethyl sulphocyanate	C ₂ H ₅ CNS	...	595		
	C ₃ H ₆ O'O''	8	589		
	C ₃ H ₁₀	8	574		
	C ₃ H ₁₆	8	573		
Cuminol.....	C ₆ H ₁₂ O'	8	568		
Furfurol.....	C ₄ H ₂ O'HCOH	?	565		
Ethyl nitrite	C ₂ H ₅ NO ₂	...	565	567	227
Allyl sulphocyanate	C ₃ H ₅ CNS	...	563		
Methylamyl ether	CH ₃ O'C ₅ H ₁₁	8	560		
Dimethylacetal (first specimen) ...	CH ₃ CH(O'CH ₃) ₂	8	554		
Octane	C ₈ H ₁₈	8	551		
	C ₄ H ₈ O'O''	9	540		
Isobutyl chloride	C ₄ H ₉ Cl	...	525		
Dimethylacetal (second specimen).	523	525	227
Anisol.....	C ₆ H ₅ O'CH ₃	9	521		
	C ₉ H ₁₂	9	515		
Allyl acetate	C ₅ H ₈ O'O''	10	503		
Epichlorhydrin.....	CH ₂ ClC ₂ H ₃ O	...	495		
	C ₅ H ₁₀ O'O''	10	495		
Carbon disulphide	CS ₂	...	491		
Isoamyl chloride	C ₅ H ₁₁ Cl	...	491		
Ethyl nitrate	C ₂ H ₅ NO ₃	...	484		
Decane	C ₁₀ H ₂₂	10	473		
Phenetol.....	C ₆ H ₅ O'C ₂ H ₅	10	471	468	217
	C ₁₀ H ₁₄	10	464		
Phenyl nitrite	C ₆ H ₅ NO ₂	...	463		
	C ₆ H ₁₂ O'O''	11	461		
Diethylacetal.....	CH ₃ CH(O'C ₂ H ₅) ₂	10	456		
Terpene (Citrene).....	C ₁₀ H ₁₆	10	450		
Benzyl chloride.....	C ₇ H ₇ Cl	...	445		

Table (continued).

Substance.	Formula.	n .	1000 kA .	1000 $\bar{k}A$.	1000 $\bar{k}An^{\frac{2}{3}}$.
Carvol	$C_{10}H_{14}O$	12?	444		
Phenyl chloride	C_6H_5Cl	...	440		
Phenyl sulphocyanate	C_6H_5CNS				
	$C_7H_{13}OO'$	12	434	435	228
Resorein	$C_6H_2(O'HCH_3)_2$	12	431		
Acetobutyric anhydride ?.....	$C_2H_3O'_3C_4H_7$	12?	431		
Cuminol	$C_{10}H_{12}O''$	13	422		
Methyl benzoate	$C_6H_5CO'O''CH_3$	13	417		
	C_2H_5Cl	...	417	414	229
Ethylene chloride	$C_2H_4Cl_2$...	412		
	$C_8H_{16}O'O''$	13	402		
Propylene chloride	$C_3H_6Cl_2$...	390		
Phosphoryl chloride.....	$POCl$...	388		
Amyl nitrate	$C_5H_{11}NO_3$...	388		
Ethyl benzoate	$C_2H_5CO'O''C_6H_5$	14	387	387	225
Paraldehyde	$C_6H_{12}O''^3$	15	379	379	231
Benzoyl chloride	$C_6H_5CO''Cl$...	373		
Ethyl oxalate... ..	$(C_2H_5)_2C_2O'_2O''_2$	16	365	365	232
Methyl paracresolate	$C_6H_3O'H \left\{ \begin{array}{l} CO'O'' \\ (CH_3)_2 \end{array} \right.$...	362		
Ethyl monochloracetate	$C_2H_5ClO'O''C_2H_5$...	359		
Benzilidene chloride.....	$C_6H_5CHCl_2$...	357		
Phosphorus sulphochloride	$PSCl$...	356		
Trichlorethane	$CH_2ClCHCl_2$				
			Mean value ...	228	

This table shows that, although the values of kA do not change at a bound on passing from one value of n to another, yet the mean value of kA taken for a given value of n found in the manner indicated varies closely inversely as the two-thirds power of n or $kA = 228/n^{\frac{2}{3}}$.

A study of the table shows that the compounds containing chlorine possess such values of kA as would make Cl equal to 5C or 6C. With regard to N in the cyanides, its value appears to diminish as the molecular weight of the compounds into which it enters increases; in the amines, on the other hand, N can with considerable accuracy be put equal to C; or if we solve strictly for n by the equation above, the value deduced for N is $\cdot 8C$. In the nitrites $NO_2 = 5\cdot 6C$ and as $O_2 = O'O'' = 5C$, N in the nitrites $= \cdot 6C$. In the nitrates NO_3 in the mean has the value $8\cdot 6C$; and as $O_3 = O'O'_2'' = 8C$, the value of N in the nitrates is $\cdot 6C$.

The small influence of the hydrogen atom in the molecule is shown by the fact that in the previous table the number of H atoms has been ignored; with a more accurate treatment the

actual value of H may be determined; but for the present, as a general result, we regard H as negligible.

If we regard the number n as measuring the actual volume of the molecule, and if we consider molecules to have similar shapes, then $n^{\frac{2}{3}}$ measures the surface of the molecule; and we can say that the parameter A varies inversely as the surface of the molecule. This interpretation is partially justified by the consideration of certain results in molecular refraction. Let i be the index of refraction of a substance whose molecule occupies a domain u , but actually fills only a volume U , and let I be the index of refraction of the matter of the molecule, then I have shown in my paper on Molecular Refraction that $(i-1)u=(I-1)U$, which is Gladstone's law. Now Landolt, Gladstone, and Brühl have shown that in general $(i-1)u$ for a molecule is the sum of certain quantities known as the refraction-equivalents characteristic of the different atoms, but that there are pronounced exceptions: thus, when an atom possesses two valencies, it has two corresponding refraction-equivalents. The same results hold therefore for $(I-1)U$; thus, if an atom has different effective valencies, it contributes different amounts to the value of $(I-1)U$ in a molecule according to its valency; thus either I or U , or both I and U , alter with the effective valency. Now we have considered n to be proportional to U ; and we have found n in the case of oxygen to vary with valency, so that the optical argument for a possible variation of U with valency agrees with the variation in the value of n which we have found. It is to be observed here that this effect of change of valency in changing n may tend to obscure the value of H as compared with C ; for in most cases a reduction in the number of H atoms is accompanied by a change of valency of the C atoms. In the study of molecular refraction we find in a large number of cases that the reduction of molecular refraction caused by the expulsion of H atoms for a molecule is almost exactly counterbalanced by the increased refraction-equivalent of C due to changed valency.

In the previous table the value obtainable from Schiff's data for certain organic bromides and iodides have been omitted, because of their exceptional character. The following table contains the values of kA for various substances.

Values of $kA \times 1000$.

Br_2	$\text{C}_2\text{H}_5\text{Br}$	$\text{C}_3\text{H}_7\text{Br}$	$\text{C}_3\text{H}_7\text{Br(Iso)}$	$\text{C}_3\text{H}_5\text{Br}$	$\text{C}_4\text{H}_9\text{Br}$
104	259	269	270	269	271
$\text{C}_5\text{H}_{11}\text{Br}$	$\text{C}_6\text{H}_5\text{Br}$	$\text{C}_7\text{H}_7\text{Br}$	$\text{C}_2\text{H}_4\text{Br}_2$	$\text{C}_3\text{H}_6\text{Br}_2$	
270	247	247	149	149	

CH_3I	$\text{C}_2\text{H}_5\text{I}$	$\text{C}_3\text{H}_7\text{I}$	$\text{C}_3\text{H}_7\text{I}(\text{Iso})$	$\text{C}_3\text{H}_5\text{I}$	$\text{C}_4\text{H}_9\text{I}$
150	159	171	170	170	176
		$\text{C}_5\text{H}_{11}\text{I}$	$\text{C}_6\text{H}_5\text{I}$		
		178	168		

In the case of bromides containing one atom of Br we see that the value of kA is stationary, or increases a little with increasing molecular weight, while in the case of the iodides the same phenomenon is more pronounced, the value increases with addition of CH_2 instead of diminishing, as it does in compounds containing C, H, O, N, and Cl. The exceptional character of these results is not surprising when we consider the large masses of the Br and I atoms. The refraction-equivalents of the organic bromides and iodides are quite regular; so that there is no ground for thinking that the volume of a Br or of an I atom varies according to the number of carbon atoms with which it is united; but the *surface* of a molecule containing Br or I united to an organic radical may perhaps no longer be considered to be proportional to the two-thirds power of the molecule on account of the pronounced want of symmetry in structure due to the preponderant atomic mass of Br and I. With this in view, and remembering that if Br and I parameter-equivalents are large, as the preceding table shows that they must be, then $228/n^{\frac{2}{3}}$ would vary slowly with the slight variation in n produced by the introduction into the molecule of a few C atoms; the effect, therefore, of dissymmetry in the building up of the organic bromides and iodides would not require to be very great to explain the exceptional character of kA in them.

In conclusion, we will see what light the law of the inverse fourth power throws on Waterston's law. If for a moment we ignore thermodynamical considerations and regard the evaporation of a liquid from the purely mechanical point of view as a change of a system of molecules attracting according to the inverse fourth power from a configuration where the density is ρ to one in which it is σ , then, as shown in my former paper (Phil. Mag. July 1887), the mutual potential energy of the molecules in the two configurations is $2\pi A\rho \log L/a$ and $2\pi A\sigma \log L/a$ respectively. If, then, for the moment we consider the latent heat-vaporization as equivalent to this change of potential energy and call the latent heat λ , we have

$$\lambda = 2\pi A\{\rho - \sigma\} \log \frac{L}{a}.$$

Now, except in the neighbourhood of the critical point, we can neglect σ , the density of the vapour, in comparison with ρ ,

that of the liquid, and can write

$$\lambda = 2\pi\Delta\rho \log \frac{L}{a}.$$

But our equation for the surface-tension gives

$$\alpha = k\Delta\rho^{\frac{5}{3}}m^{\frac{1}{3}};$$

$$\therefore \frac{m\lambda}{\frac{\alpha m^{\frac{2}{3}}}{\rho^{\frac{2}{3}}}} = \frac{2\pi \log \frac{L}{a}}{k} = \text{constant}.$$

This is Waterston's law. Now according to thermodynamical principles the latent heat of evaporation without performance of external work is

$$\int_{\frac{1}{\sigma}}^{\frac{1}{\rho}} \theta \frac{\partial p}{\partial \theta} dv,$$

where θ is the absolute temperature at which evaporation takes place. This cannot be evaluated till we know the characteristic equation for fluids, which bridges in a complete manner the gap between the liquid and the gaseous states. But if we make the assumption that the latent heats of all fluids at their boiling-points are approximately the same fraction of the change of potential energy of the molecules due to evaporation, then Waterston's law would still be an approximate deduction from the law of the inverse fourth power. And this assumption is a natural one, seeing that Ramsay and Young have pointed out that the external work done during evaporation at the ordinary boiling-point is approximately the same fraction of the total latent heat for all liquids; our assumption is, that the fraction of the latent heat due to thermodynamical adjustment is for all liquids the same fraction of the change of mechanical energy. However, that the assumption is not a sound one is shown by the roughness of the approach to constancy of the following values of $\frac{m\lambda\rho^{\frac{2}{3}}}{\alpha m^{\frac{2}{3}}}$. The alcohols and acids of the fatty series are omitted because of their not following Eötvös's law.

C_5H_{10}	C_6H_6	$C_{10}H_{16}$	$CHCl_3$	CCl_4	C_2H_5Br	C_2H_5I	$C_2H_4Br_2$
6.7	6.2	4.5	5.8	6.2	6.1	5.9	6.7

$C_5H_{11}Cl$	$C_5H_{11}Br$	$C_5H_{11}I$	CH_3I	C_3H_6O (acetone)
6.9	6.1	4.9	6.7	4.7

CCl_3COH	$(\text{C}_2\text{H}_5)_2\text{C}_2\text{O}_4$	$(\text{CH}_3\text{CO})_2\text{O}$	$\text{C}_3\text{H}_6\text{O}_2$	$\text{C}_4\text{H}_8\text{O}_2$
5.3	4.4	6.7	5.5	5.4
			Mean.	Mean.

$\text{C}_5\text{H}_{10}\text{O}_2$	$\text{C}_6\text{H}_{12}\text{O}_2$	$\text{C}_7\text{H}_{14}\text{O}_2$	$\text{C}_8\text{H}_{16}\text{O}_2$
5.3	6.0	6.5	6.9
Mean.	Mean.	Mean.	Mean.

The previous numbers show that Waterston's law, while representing a remarkable attempt at the time it was announced to connect latent heat, surface-tension, and molecular volume, is far from being verified by experimental data; and they show the danger of pushing the idea of "correspondence" too far. In fact, it would appear that the manner in which Eötvös deduces his law theoretically from the principle of "correspondence" only happens by chance to give a rigorous law, as the same principle applied to latent heats and surface-energy leads to a law so far from rigorous as Waterston's has been shown to be.

In the preceding pages we have argued from Eötvös's empirical law as basis; but probably, when the dynamical meaning of temperature in liquids has been made clear, Eötvös's law will be deduced in the following manner.

The temperature of a liquid subjected to small constant pressure is such a property of the molecule of the liquid, that its rate of variation with the translatory kinetic energy of the molecule is the same for all molecules; but as external virial is negligible under these circumstances, we have the rate of variation with temperature of internal virial multiplied by molecular mass proportional to the rate of variation of the translatory kinetic energy of the molecule, which is the same for all molecules. But according to the law of the inverse fourth power, internal virial multiplied by the mass of the molecule is proportional to surface-tension multiplied by the two-thirds power of the molecular domain, whence Eötvös's law follows, that the rate of variation with temperature of the product of surface-tension and the two-thirds power of the molecular domain is the same for nearly all liquids.

In the present paper we have been occupied only with attractions between similar molecules; and there is too little experimental material to allow of our testing any hypothesis as to the parameter of molecular attraction in the case of dissimilar ones; but it seems probable that the parameter will be inversely proportional to the product of the square roots of the surfaces of the two molecules.

XXXVII. *Note on the Measurement of Resistance.*

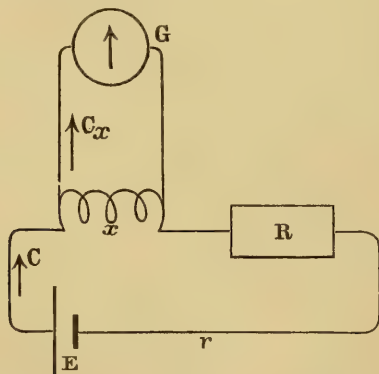
By Dr. J. W. W. WAGHORN*.

IT is well known that an unknown resistance x can be determined in terms of a known resistance R , if both are arranged in series with a cell, and if the deflexions of an electrometer be observed when connected in turn to x and to R .

If C is the current in the circuit, the differences of potential at the terminals of x and of R are respectively Cx and CR .

It is also well known that any voltmeter may be substituted instead of the electrometer, if the resistance of the voltmeter-circuit is very large compared to x and R .

But it may not have been generally observed that the same method may be employed whatever be the relative resistances of the voltmeter and the conductors ; and is true even if the usual conditions are reversed, so that the voltmeter-resistance is very small compared to x and R . Any galvanometer may take the place of the voltmeter.



Let G be the resistance of the galvanometer ;

E be the E.M.F. of the battery ;

r be the resistance of the battery and connecting-wires.

Let C be the strength of the current on the undivided part of the circuit ;

C_x be the strength of the current through the galvanometer when its terminals are connected to x ;

C_R be the strength of the current through the galvanometer when its terminals are connected to R .

* Communicated by the Physical Society : read February 23, 1889.

Then

$$C = \frac{E}{R + r + \frac{Gx}{G+x}},$$

and

$$\begin{aligned} C_x &= C \times \frac{x}{G+x} = \frac{E}{R + r + \frac{Gx}{G+x}} \times \frac{x}{G+x} \\ &= \frac{Ex}{RG + Gx + rG + rx}. \end{aligned}$$

Also, substituting R for x ,

$$C_R = \frac{ER}{RG + Gx + rG + rR}.$$

If x and R are large compared to r , the denominators of the above fractions will be practically identical; and the approximation will be still closer if, as would usually occur in practice, x is not very different from R .

The sensitiveness of the method is practically the same as that obtained in the ordinary "substitution" process of determining, but has the small advantage over the latter of not requiring a knowledge of the resistance of the galvanometer.

XXXVIII. On the Dimensions of Electromagnetic Units.

By Prof. G. F. FITZGERALD, F.R.S.*

SOME attention has lately been called to the question of the dimensions of electromagnetic units, but the following obvious suggestion seems to have escaped notice.

The electrostatic system of units may be described as one in which electric inductive capacity is *assumed* to have zero dimensions and the electromagnetic system as one in which the magnetic inductive capacity is assumed to have zero dimensions. Now if we take a system in which the dimensions of both these quantities are the same, and of the dimensions of a slowness,

i. e. the inverse of a velocity, $\left[\frac{T}{L}\right]$, the two systems become identical as regards dimensions, and differ only by a numerical coefficient just as centimetres and kilometres do. There seems a naturalness in this result that justifies the assumption that these inductive capacities are really of the nature of a slowness. It seems possible that they are related to the reciprocal of the square root of the mean energy of turbulence of the æther.

* Communicated by the Physical Society: read February 23, 1889.

XXXIX. *On the Electromagnetic Effects due to the Motion of Electrification through a Dielectric.* By OLIVER HEAVISIDE*.

1. **T**HE following paper consists of, First, a short discussion of the theory of the *slow* motion of an electric charge through a dielectric, having for object the possible correction of previously published results. Secondly, a discussion of the theory of the electromagnetic effects due to motion of a charge at any speed, with the development of the complete solution in finite form when the motion is steady and rectilinear. Thirdly, a few simple illustrations of the last when the charge is distributed.

Given a steady electric field in a dielectric, due to electrification. It is sufficient to consider a charge q at a point, as we may readily extend results later. If this charge be shifted from one position to another, the displacement varies. In accordance, therefore, with Maxwell's inimitable theory of a dielectric, there is electric current produced. Its time-integral, which is the total change in the displacement, admits of no question; but it is by no means an elementary matter to settle its rate of change in general, or the electric current. But should the speed of the moving charge be only a very small fraction of that of the propagation of disturbances, or that of light, it is clear that the accommodation of the displacement to the new positions which are assumed by the charge during its motion is practically instantaneous in its neighbourhood, so that we may imagine the charge to carry about its stationary field of force rigidly attached to it. This fixation of the displacement at any moment definitely fixes the displacement-current. We at once find, however, that to close the current requires us to regard the moving charge itself as a current-element, of moment equal to the charge multiplied by its velocity; understanding by moment, in the case of a distributed current, the product of current-density and volume. The necessity of regarding the moving charge as an element of the "true current" may be also concluded by simply considering that when a charge q is conveyed *into* any region, an equal displacement simultaneously leaves it through its boundary.

Knowing the electric current, the magnetic force to correspond becomes definitely known if the distribution of inductivity be given; and when this is constant everywhere, as we shall suppose now and later, the magnetic force is simply

* Communicated by the Author.

the vector of no divergence whose curl is 4π times the electric current; or the vector-potential of the curl of the current; or the curl of the vector-potential of the current, &c. &c. Thus, as found by J. J. Thomson*, the magnetic field of a charge moving at a speed which is a small fraction of that of light is that which is commonly ascribed to a current-element itself. I think it, however, preferable to regard the magnetic field as the primary object of attention; or else to regard the complete system of closed current derived from it by taking its curl as the unit, forming what we may term a rational current-element, inasmuch as it is not a mere mathematical abstraction, but is a complete dynamical system involving definite forces and energy.

2. Let the axis of z be the line of motion of the charge q at the speed u ; then the lines of magnetic force \mathbf{H} are circles centred upon the axis, in planes perpendicular to it, and its tensor H at distance r from the charge, the line \mathbf{r} making an angle θ with the axis, is given by

$$H = \frac{q}{r^2} u \sin \theta = c E \nu v, \quad . \quad . \quad . \quad . \quad (1)$$

where $\nu = \sin \theta$, E the intensity of the radial electric force, c the permittivity such that $\mu_0 c v^2 = 1$, if μ_0 is the other specific quality of the medium, its inductivity, and v is the speed of propagation.

Since, under the circumstance supposed of u/v being very small, the alteration in the electric field is insensible, and the lines of \mathbf{E} are radial, we may terminate the fields represented by (1) at any distance $r = a$ from the origin. We then obtain the solution in the case of a charge q upon the surface of a conducting sphere of radius a , moving at speed u . This realization of the problem makes the electric and magnetic energies finite. Whilst, however, agreeing with J. J. Thomson in the fundamentals, I have been unable to corroborate some of his details; and since some of his results have been recently repeated by him in another place†, it may be desirable to state the changes I propose, before proceeding to the case of a charge moving at any speed.

3. First, as regards the magnetic energy, say T . This is the space-summation $\Sigma \mu_0 H^2 / 8\pi$; or, by (1)‡,

$$T = \frac{\mu_0 q^2 u^2}{8\pi} \iiint \frac{\nu^2}{r^2} dr d\mu d\phi = \frac{\mu_0 q^2 u^2}{3a}. \quad . \quad . \quad . \quad (2)$$

* Phil. Mag. April 1881.

† 'Applications of Dynamics to Physics and Chemistry,' chap. iv. pp. 31 to 37.

‡ 'Electrician,' Jan. 24, 1885, p. 220.

The limits are such as include all space outside the sphere $r=a$. The coefficient $\frac{1}{3}$ replaces $\frac{2}{15}$.

4. Next, as regards the mutual magnetic energy M of the moving charge and any external magnetic field. This is the space-summation $\Sigma \mu_0 \mathbf{H}_0 \mathbf{H} / 4\pi$, if \mathbf{H}_0 is the external field ; and, by a well-known transformation, it is equivalent to $\Sigma \mathbf{A}_0 \mathbf{\Gamma}$, if \mathbf{A}_0 is any vector whose curl is $\mu_0 \mathbf{H}_0$, whilst $\mathbf{\Gamma}$ is the current-density of the moving system. Further, if we choose \mathbf{A}_0 to have no divergence, the polar part of $\mathbf{\Gamma}$ will contribute nothing to the summation, so that we are reduced to the volume-integral of the scalar product of the divergenceless \mathbf{A}_0 of the one system and the density of the convection-current in the other. Or, in the present case, with a single moving charge at a point, we have simply the scalar product $\mathbf{A}_0 u q$ to represent the mutual magnetic energy ; or

$$M = \mathbf{A}_0 u q, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which is double J. J. Thomson's result.

5. When, therefore, we derive from (3) the mechanical force on the moving charge due to the external magnetic field, we obtain simply Maxwell's "electromagnetic force" on a current-element, the vector product of the moment of the current and the induction of the external field ; or, if \mathbf{F} is this mechanical force,

$$\mathbf{F} = \mu_0 q \mathbf{V} u \mathbf{H}_0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which is also double J. J. Thomson's result. Notice that in the application of the "electromagnetic force" formula, it is the moment of the convection-current that occurs. This is not the same as the moment of the true current, which varies according to circumstances ; for instance, in the case of a small dielectric sphere uniformly electrified throughout its volume, the moment of the true current would be only $\frac{2}{3}$ of that of the convection-current.

The application of Lagrange's equation of motion to (3) also gives the force on q due to the electric field so far as it can depend on M ; that is, a force

$$-q \frac{d\mathbf{A}_0}{dt},$$

where the time-variation due to all causes must be reckoned, except that due to the motion of q itself, which is allowed for in (4). And besides this, there may be electric force not derivable from \mathbf{A}_0 , viz.

$$-q \nabla \Psi_0,$$

where Ψ_0 is the scalar potential companion to \mathbf{A}_0 .

6. Now if the external field be that of another moving charge, we shall obtain the mutual magnetic energy from (3) by letting \mathbf{A}_0 be the vector-potential of the current in the second moving system, constructed so as to have no divergence. Now the vector-potential of the convection-current qu is simply qu/r ; this is sufficient to obtain the magnetic force by curling; but if used to calculate the mutual energy, the space-summation would have to include every element of current in the other system. To make the vector-potential divergenceless, and so be able to abolish this work, we must add on to qu/r the vector-potential of the displacement current to correspond. Now the complete current may be considered to consist of a linear element qu having two poles; a radial current outward from the + pole in which the current-density is $qu/4\pi r_1^2$; and a radial current inward to the - pole, in which the current-density is $-qu/4\pi r_2^2$; where r_1 and r_2 are the distances of any point from the poles. The vector-potentials of these currents are also radial, and their tensors are $\frac{1}{2}qu$ and $-\frac{1}{2}qu$. We have now merely to find their resultant when the linear element is indefinitely shortened, add on to the former qu/r , and multiply by μ_0 , to obtain the complete divergenceless vector-potential of qu , viz. :—

$$\mathbf{A} = \mu_0 \frac{q}{r} \left(\mathbf{u} - \frac{1}{2} u \nabla \frac{dr}{ds} \right), \dots \dots \dots (5)$$

where r is the distance from q to the point P when \mathbf{A} is reckoned, and the differentiation is to s the axis of the convection-current. Both it and the space-variation are taken at P. The tensor of \mathbf{u} is u . Though different and simpler in form (apart from the use of vectors) this vector-potential is, I believe, really the same as the one used by J. J. Thomson. From it we at once find, by the method described in § 4, the mutual energy of a pair of point-charges q_1 and q_2 moving at velocities \mathbf{u}_1 and \mathbf{u}_2 to be

$$M = \frac{\mu_0 q_1 q_2}{r} \left(\mathbf{u}_1 \mathbf{u}_2 - \frac{1}{2} u_1 u_2 \frac{d^2 r}{ds_1 ds_2} \right), \dots \dots \dots (6)$$

when at distance r apart. Both axial differentiations are to be effected at one end of the line \mathbf{r} .

As an alternative form, let ϵ be the angle between \mathbf{u}_1 and \mathbf{u}_2 , and let the differentiation to s_1 be at ds_1 , that to s_2 at ds_2 , as in the German investigations relating to current-elements; then *

$$M = \frac{\mu_0 q_1 q_2 u_1 u_2}{r} \left(\cos \epsilon + \frac{1}{2} \frac{d^2 r}{ds_1 ds_2} \right). \dots \dots \dots (7)$$

* 'Electrician,' Dec. 28, 1888, p. 230.

Another form, to render its meaning plainer. Let λ_1, μ_1, ν_1 and λ_2, μ_2, ν_2 be the direction-cosines of the elements referred to rectangular axes, with the x -axis, to which λ_1 and λ_2 refer, chosen as the line joining the elements. Then*

$$M = \frac{\mu_0 q_1 q_2 u_1 u_2}{2r} (2\lambda_1 \lambda_2 + \mu_1 \mu_2 + \nu_1 \nu_2). \quad . \quad . \quad (8)$$

J. J. Thomson's estimate is †

$$M = \frac{1}{3} \mu_0 q_1 q_2 u_1 u_2 \frac{\cos \epsilon}{r} . \quad . \quad . \quad . \quad . \quad (9)$$

Comparing this with (8) we see that there is a notable difference.

7. The mutual energy being different, the forces on the charges, as derived by J. J. Thomson by the use of Lagrange's equations, will be different. When the speeds are constant, we shall have simply the before-described vector product (4) for the "electromagnetic force;" or

$$\mathbf{F}_1 = \mu_0 q_1 \mathbf{V} u_1 \mathbf{H}_2, \quad \mathbf{F}_2 = \mu_0 q_2 \mathbf{V} u_2 \mathbf{H}_1 . \quad . \quad . \quad (10)$$

if \mathbf{F}_1 is the electromagnetic force on the first and \mathbf{F}_2 that on the second element, whilst \mathbf{H}_1 and \mathbf{H}_2 are the magnetic forces. Similar changes are needed in the other parts of the complete mechanical forces.

It may be remarked that (if my calculations are correct) equation (7) or its equivalents expresses the mutual energy of any two rational current-elements [see § 1] in a medium of uniform inductivity, of moments $q_1 u_1$ and $q_2 u_2$, whether the currents be of displacement, or conduction, or convection, or all mixed, it being in fact the mutual energy of a pair of definite magnetic fields. But, since the hypothesis of instantaneous action is expressly involved in the above, the application of (7) is of a limited nature.

8. Now leaving behind altogether the subject of current-elements, in the investigation of which one is liable to be led away from physical considerations and become involved in mere exercises in differential coefficients, and coming to the question of the electromagnetic effects of a charge moving in any way, I have been agreeably surprised to find that my solution in the case of steady rectilinear motion, originally an infinite series of corrections, easily reduces to a very simple and interesting finite form, provided u be not greater than v . Only when $u > v$ is there any difficulty. We must first settle

* 'Electrician,' Jan 24, 1885, p. 221.

† 'Applications of Dynamics to Physics and Chemistry,' chap. iv.; and Phil. Mag. April 1881.

upon what basis to work. First the Faraday-law (p standing for d/dt),

$$-\text{curl } \mathbf{E} = \mu_0 p \mathbf{H}, \quad . \quad . \quad . \quad . \quad . \quad (11)$$

requires no change when there is moving electrification. But the analogous law of Maxwell, which I understand to be really a *definition* of electric current in terms of magnetic force, (or a doctrine), requires modification if the true current is to be

$$\mathbf{C} + p\mathbf{D} + \rho\mathbf{u}; \quad . \quad . \quad . \quad . \quad . \quad (12)$$

viz. the sum of conduction-current, displacement-current, and convection-current $\rho\mathbf{u}$, where ρ is the volume-density of electrification. The addition of the term $\rho\mathbf{u}$ was, I believe, proposed by G. F. Fitzgerald*.

[This was not meant exactly for a new proposal, being in fact after Rowland's experiments; besides which, Maxwell was well acquainted with the idea of a convection-current. But what is very strange is that Maxwell, who insisted so strongly upon his doctrine of the *quasi*-incompressibility of electricity, never formulated the convection-current in his treatise. Now Prof. Fitzgerald pointed out that if Maxwell, in his equation of mechanical force,

$$\mathbf{F} = \mathbf{V}\mathbf{C}\mathbf{B} - e\nabla\Psi - m\nabla\Omega,$$

had written \mathbf{E} for $-\nabla\Psi$, as it is obvious he should have done, then the inclusion of convection-current in the true current would have followed naturally. (Here \mathbf{C} is the true current, \mathbf{B} the induction, e the density of electrification, m that of imaginary magnetic matter, Ψ the electrostatic and Ω the magnetic potential, and \mathbf{E} the real electric force.)

Now to this remark I have to add that it is as unjustifiable to derive \mathbf{H} from Ω as \mathbf{E} from Ψ ; that is, in general, the magnetic force is not the slope of a scalar potential; so, for $-\nabla\Omega$ we should write \mathbf{H} , the real magnetic force.

But this is not all. There is possibly a fourth term in \mathbf{F} , expressed by $4\pi\mathbf{V}\mathbf{D}\mathbf{G}$, where \mathbf{D} is the displacement and \mathbf{G} the magnetic current; I have termed this force the "magneto-electric force," because it is the analogue of Maxwell's "electromagnetic force" $\mathbf{V}\mathbf{C}\mathbf{B}$. Perhaps the simplest way of deriving it is from Maxwell's electric stress, which was the method I followed †.

Thus, in a homogeneous nonconducting dielectric free from electrification and magnetization, the mechanical force is the sum of the "electromagnetic" and the "magnetoelec-

* Brit. Assoc., Southport, 1883.

† "El. Mag. Ind. and its Prop." xxii. 'Electrician,' Jan. 15, 1886, p. 187.

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 tric," and is given by

$$\mathbf{F} = \frac{1}{v^2} \frac{d\mathbf{W}}{dt},$$

where $\mathbf{W} = V\mathbf{E}\mathbf{H}/4\pi$ is the transfer-of-energy vector.

It must, however, be confessed that the real distribution of the stresses, and therefore of the forces, is open to question. And when æther is the medium, the mechanical force in it, as for instance in a light-wave, or in a wave sent along a telegraph-circuit, is not easily to be interpreted.]

The companion to (11) in a nonconducting dielectric is now

$$\text{curl } \mathbf{H} = c\rho\mathbf{E} + 4\pi\rho\mathbf{u}. \quad (13)$$

Eliminate \mathbf{E} between (11) and (13), remembering that \mathbf{H} has no divergence, because μ_0 is constant, and we get

$$(p^2/v^2 - \nabla^2)\mathbf{H} = \text{curl } 4\pi\rho\mathbf{u}, \quad (14)$$

the characteristic of \mathbf{H} . Here $\nabla^2 = d^2/dx^2 + \dots$, as usual.

Comparing (14) with the characteristic of \mathbf{H} when there is impressed force e instead of electrification ρ , which is

$$(p^2/v^2 - \nabla^2)\mathbf{H} = \text{curl } c\rho e,$$

we see that $\rho\mathbf{u}$ becomes $c\rho e/4\pi$. We may therefore regard convection-current as *impressed* electric current. From this comparison also, we may see that an infinite plane sheet of electrification of uniform density cannot produce magnetic force by motion perpendicular to its plane. Also we see that the sources of disturbances when ρ is moved are the places where $\rho\mathbf{u}$ has curl; for example, a dielectric sphere uniformly filled with electrification (which is imaginable), when moved, starts the magnetic force solely upon its boundary.

The presence of "curl" on the right side tells us, as a matter of mathematical simplicity, to make \mathbf{H}/curl the variable. Let

$$\mathbf{H} = \text{curl } \mathbf{A}, \quad (15)$$

and calculate \mathbf{A} , which may be any vector satisfying (15). Its characteristic is

$$(p^2/v^2 - \nabla^2)\mathbf{A} = 4\pi\rho\mathbf{u}. \quad (16)$$

The divergence of \mathbf{A} is of no moment, and it is only vexatious complication to introduce Ψ . The time-rate of decrease of \mathbf{A} is not the real distribution of electric force, which has to be found by the additional datum

$$\text{div } c\mathbf{E} = 4\pi\rho, \quad (17)$$

where \mathbf{E} is the real force.

9. "Symbolically" expressed, the solution of (16) is

$$\mathbf{A} = \frac{4\pi\rho\mathbf{u}}{p^2/v^2 - \nabla^2} = \frac{-4\pi\rho\mathbf{u}/\nabla^2}{1 - p^2/v^2 \nabla^2}. \quad (18)$$

Here the numerator of the fraction to the right is the vector-potential of the convection-current. Calling it \mathbf{A}_0 , we have

$$\mathbf{A}_0 = \frac{4\pi\rho\mathbf{u}}{-\nabla^2} = \Sigma \frac{\rho\mathbf{u}}{r}. \quad (19)$$

Inserting in (18) and expanding, we have

$$\mathbf{A} = \{1 + (p/v\nabla)^2 + (p/v\nabla)^4 + \dots\}\mathbf{A}_0. \quad (20)$$

Given then $\rho\mathbf{u}$ as a function of position and time, \mathbf{A}_0 is known by (19), and (20) finds \mathbf{A} , whilst (15) finds \mathbf{H} .

10. When the motion of the electrification is all in one direction, say parallel to the z -axis, \mathbf{u} , \mathbf{A}_0 , and \mathbf{A} are all parallel to this axis, so that we need only consider their tensors. When there is simply one charge q at a point, we have

$$A_0 = qu/r,$$

and (20) becomes

$$A = q\{1 + (p/v\nabla)^2 + (p/v\nabla)^4 + \dots\}(u/r) \quad (21)$$

at distance r from q . When the motion is steady, and the whole electromagnetic field is ultimately steady with respect to the moving charge, we shall have, taking it as origin,

$$p = -u(d/dz) = -uD$$

for brevity; so that

$$A = qu\{1 + (uD/v\nabla)^2 + (uD/v\nabla)^4 + \dots\}r^{-1}. \quad (22)$$

Now the property

$$\nabla^2 r^{n+2} = (n+2)(n+3)r^n \quad (23)$$

brings (22) to

$$A = qu\left\{\frac{1}{r} + \frac{u^2}{v^2}D^2\frac{r}{2!} + \frac{u^4}{v^4}D^4\frac{r^3}{4!} + \dots\right\}; \quad (24)$$

and the property

$$D^{2n}r^{2n-1} = 1^2.3^2.5^2\dots(2n-1)^2r^{2n}/r, \quad (25)$$

where $\nu = \sin\theta$, θ being the angle between \mathbf{r} and the axis, brings (24) to

$$A = \frac{qu}{r}\left\{1 + \frac{u^2}{v^2}\frac{\nu^2}{2}\left(1 + \frac{u^2}{v^2}\frac{3}{4}\nu^2\left(1 + \frac{u^2}{v^2}\frac{5}{6}\nu^2\left(1 + \dots\right)\right)\right)\right\}; \quad (26)$$

which, by the Binomial Theorem, is the same as

$$A = (qu/r)\{1 - u^2\nu^2/v^2\}^{-\frac{1}{2}}, \quad (27)$$

the required solution.

11. To derive \mathbf{H} , the tensor of the circular \mathbf{H} , let $r\nu = h$, the distance from the axis. Then, by (15),

$$\mathbf{H} = -\frac{d\mathbf{A}}{dh} = -\nu\frac{d\mathbf{A}}{dr} + \frac{\mu\nu}{r}\frac{d\mathbf{A}}{d\mu} = \frac{qu\nu}{r^2}\left(1 + \mu\frac{d}{d\mu}\right)\left(1 - \frac{u^2}{v^2}\nu^2\right)^{-\frac{1}{2}}, \quad (28)$$

by (27), if $\mu = \cos \theta$. Performing the differentiation, and also getting out \mathbf{E} the tensor of the electric force, we have the final result that the electromagnetic field is fully given by*

$$c\mathbf{E} = \frac{q}{r^2} \cdot \frac{1 - u^2/v^2}{(1 - u^2 v^2/v^2)^{\frac{3}{2}}}, \quad \mathbf{H} = c\mathbf{E}u\mathbf{v}, \quad . \quad . \quad (29)$$

with the additional information that \mathbf{E} is radial and \mathbf{H} circular.

Now, as regards Ψ , if we bring it in, we have only got to take it out again. When the speed is very slow we may regard the electric field as given by $-\nabla\Psi$ plus a small correcting vector, which we may call the electric force of inertia. But to show the *physical* inanity of Ψ , go to the other extreme, and let u nearly equal v . It is now the electric force of inertia (supposed) that equals $+\nabla\Psi$ nearly (except about the equatorial plane), and its sole utility or function is to cancel the other $-\nabla\Psi$ of the (supposed) electrostatic field. It is surely impossible to attach any physical meaning to Ψ and to propagate it, for we require two Ψ 's, one to cancel the other, and both propagated infinitely rapidly.

As the speed increases, the electromagnetic field concentrates itself more and more about the equatorial plane, $\theta = \frac{1}{2}\pi$. To give an idea of the accumulation, let $u^2/v^2 = .99$. Then $c\mathbf{E}$ is .01 of the normal value q/r^2 at the pole, and 10 times the normal value at the equator. The latitude where the value is normal is given by

$$v = (v/u)[1 - (1 - u^2/v^2)^{\frac{2}{3}}]^{\frac{1}{2}}. \quad . \quad . \quad . \quad (30)$$

12. When $u = v$, the solution (29) becomes a plane electromagnetic wave, \mathbf{E} and \mathbf{H} being zero everywhere except in the equatorial plane. As, however, the values of \mathbf{E} and \mathbf{H} are infinite, distribute the charge along a straight line moving in its own line, and let the linear-density be q . The solution is then †

$$\mathbf{H} = \mathbf{E}cv = 2qv/r \quad . \quad . \quad . \quad (31)$$

at distance r from the line, between the two planes through the ends of the line perpendicular to it, and zero elsewhere.

To further realize, let the field terminate internally at $r = a$, giving a cylindrical-surface distribution of electrification, and terminate the tubes of displacement externally upon a coaxial cylindrical surface; we then produce a real electromagnetic plane wave with electrification, and of finite energy. We have supposed the electrification to be carried through the dielectric at speed v , to keep up with the wave, which would of course

* 'Electrician,' Dec. 7, 1888, p. 148.

† Ibid. Nov. 23, 1888, p. 84.

break up if the charge were stopped. But if perfectly-conducting surfaces be given on which to terminate the displacement, the natural motion of the wave will itself carry the electrification along them. In fact we now have the rudimentary telegraph-circuit, with no allowance made for absorption of energy in the wires, and the consequent distortion. If the conductors be not coaxial, we only alter the distribution of the displacement and induction, without affecting the propagation without distortion*.

If we now make the medium conduct electrically, and likewise magnetically, with equal rates of subsidence, we shall have the same solutions, with a time-factor $\epsilon^{-\rho t}$ producing ultimate subsidence to zero; and, with only the real electric conductivity in the medium the wave is running through, it will approximately cancel the distortion produced by the resistance of the wires the wave is passing over when this resistance has a certain value†. We should notice, however, that it could not do so perfectly, even if the magnetic retardation in the wires due to diffusion were zero; because in the case of the unreal magnetic conductivity its correcting influence is where it is wanted to be, in the body of the wave; whereas in the case of the wires, their resistance, correcting the distortion due to the external conductivity, is outside the wave; so that we virtually assume instantaneous propagation laterally from the wires of *their* correcting influence in the elementary theory of propagation along a telegraph-circuit which is symbolized by the equations

$$-\frac{dV}{dz} = (R + Lp)C, \quad -\frac{dC}{dz} = (K + Sp)V, \quad . \quad (32)$$

where R, L, K, and S are the resistance, inductance, leakage-conductance, and permittance per unit length of circuit, C the current, and V what I, for convenience, term the potential-difference, but which I have expressly disclaimed‡ to represent the electrostatic difference of potential, and have shown to represent the transverse E.M.F. or line-integral of the electric force across the circuit from wire to wire, including the electric force of inertia. Now in case of great distortion, as in a long submarine cable, this V approximates towards the electrostatic potential-difference, which it is in Sir W. Thomson's diffusion theory; but in case of little distortion, as

* 'Electrician,' Jan. 10, 1885. Also "Self-Induction of Wires," part iv. Phil. Mag. Nov. 1886.

† "Electromagnetic Waves," § 6, Phil. Mag. Feb. 1888. 'Electrician,' June 1887.

‡ "Self-Induction of Wires," part ii. Phil. Mag. Sept. 1886.

in telephony through circuits of low resistance and large inductance, there may be a wide difference between my V and that of the electrostatic force. Consider, for instance, the extreme case of an isolated plane-wave disturbance with no spreading-out of the tubes of displacement. At the boundaries of the disturbance the difference between V and the electrostatic difference of potential is great.

But it is worth noticing, as a rather remarkable circumstance, that when we derive the system (32) by elementary considerations, viz. by extending the diffusion-system by the addition of the E.M.F. of inertia and leakage-current, we apparently as a matter of course take V to mean the same as in the diffusion-system. The resulting equations are correct, and yet the assumption is certainly wrong. The true way appears to be that given by me in the paper last referred to, by considering the line-integral of electric force in a closed curve. We cannot, indeed, make a separation of the electric force of inertia from $-\Delta\Psi$ without some assumption, though the former is quite definite when the latter is suitably defined. But, and this is the really important matter, it would be in the highest degree inconvenient, and lead to much complication and some confusion, to split V into two components, in other words, to bring in Ψ and A .

In thus running down Ψ , I am by no means forgetful of its utility in other cases. But it has perhaps been greatly misused. The clearest course to pursue appears to me to invariably make E and H the primary objects of attention, and only use potentials when they naturally suggest themselves as labour-saving appliances.

13. Returning to the solutions (29), the following are the special tests of their accuracy. Let E_1 and E_2 be the z and h components of E . Then, by (11) and (13), with the special meaning assumed by p , we have

$$\left. \begin{aligned} \frac{1}{h} \frac{d}{dh} hH &= -cu \frac{dE_1}{dz}, \\ -\frac{dH}{dz} &= -cu \frac{dE_2}{dz}, \quad \text{or } H = cuE_2, \\ \frac{dE_1}{dh} - \frac{dE_2}{dz} &= -\mu_0 u \frac{dH}{dz}, \quad \text{or } \frac{dE_1}{dh} = \left(1 - \frac{u^2}{v^2}\right) \frac{dE_2}{dz}. \end{aligned} \right\} \quad (33)$$

In addition to satisfying these equations, the displacement outward through any spherical surface centred at the charge may be verified to be q ; this completes the test of the accuracy of (29).

But (33) are not limited to the case of a single point-charge, being true outside the electrification when there is symmetry with respect to the z -axis, and the electrification is all moving parallel to it at speed u .

When $u=v$, $E_1=0$, and $E_2=E=\mu v H$, so that we reduce to

$$\frac{1}{h} \frac{d}{dh} hH=0 \quad . \quad . \quad . \quad . \quad . \quad (34)$$

outside the electrification. Thus, if the electrification is on the axis of z , we have

$$E/\mu v=H=2qv/r, \quad . \quad . \quad . \quad . \quad . \quad (35)$$

differing from (31) only in that q , the linear density, may be any function of z .

14. If, in the solutions (29), we terminate the fields internally at $r=a$, the perpendicularity of \mathbf{E} and the tangentiality of \mathbf{H} to the surface show that (29) represents the solutions in the case of a perfectly conducting sphere of radius a , moving steadily along the z -axis at the speed u , and possessing a total charge q . The energy is now finite. Let U be the total electric and T the total magnetic energy. By space-integration of the squares of \mathbf{E} and \mathbf{H} we find that they are given by

$$U = \frac{q^2}{2ca} \cdot \frac{1-u^2/v^2}{4} \left[1 + \frac{\frac{3}{2}}{1-u^2/v^2} + \frac{\frac{3}{2} \tan^{-1} \frac{u/v}{(1-u^2/v^2)^{\frac{3}{2}}}}{(u/v)(1-u^2/v^2)^{\frac{3}{2}}} \right], \quad (36)$$

$$T = \frac{q^2}{2ca} \cdot \frac{1-u^2/v^2}{4} \left[1 + \frac{2u^2/v^2 - \frac{1}{2}}{1-u^2/v^2} + \frac{(2u^2/v^2 - \frac{1}{2}) \tan^{-1} \frac{u/v}{(1-u^2/v^2)^{\frac{3}{2}}}}{(u/v)(1-u^2/v^2)^{\frac{3}{2}}} \right], \quad (37)$$

in which $u < v$. When $u=v$, with accumulation of the charge at the equator of the sphere, we have infinite values, and it appears to be only possible to have finite values by making a zone at the equator cylindrical instead of spherical. The expression for T in (37) looks quite wrong; but it correctly reduces to that of equation (2) when u/v is infinitely small.

15. The question now suggests itself, What is the state of things when $u > v$? It is clear, in the first place, that there can be no disturbance at all in front of the moving charge (at a point, for simplicity). Next, considering that the spherical waves emitted by the charge in its motion along the z -axis travel at speed v , the locus of their fronts is a conical surface whose apex is at the charge itself, whose axis is that of z , and whose semiangle θ is given by

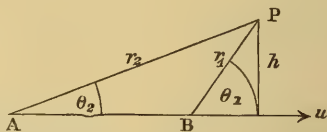
$$\sin \theta = v/u. \quad . \quad . \quad . \quad . \quad . \quad (38)$$

The whole displacement, of amount q , should therefore lie within this cone. And since the moving charge is a convection-current qu , the displacement-current should be towards the apex in the axial portion of the cone, and change sign at some unknown distance, so as to be away from the apex either in the outer part of the cone or else upon its boundary. The pulling back of the charge by the electric stress would require the continued application of impressed force to keep up the motion, and its activity would be accounted for by the continuous addition made to the energy in the cone; for the transfer of energy on its boundary is perpendicularly outward, and the field at the apex is being continuously renewed.

The above general reasoning seems plausible enough, but I cannot find any solution to correspond that will satisfy all the necessary conditions. It is clear that (29) will not do when $u > v$. Nor is it of any use to change the sign of the quantity under the radical, when needed, to make real. It is suggested that whilst there should be a definite solution, there cannot be one representing a *steady* condition of \mathbf{E} and \mathbf{H} with respect to the moving charge. As regards physical possibility, in connexion with the structure of the æther, that is not in question.

16. Let us now derive from (29), or from (27), the results in some cases of distributed electrification, in steady rectilinear motion. The integrations to be effected being all of an elementary character, it is not necessary to give the working.

First, let a straight line AB be charged to linear density q , and be in motion at speed u in its own line from left to right. Then at P we shall have



$$A = qu \log \left(\frac{r_1}{r_2} \cdot \frac{\mu_1 + (1 - \nu_1^2 u^2/v^2)^{\frac{1}{2}}}{\mu_2 + (1 - \nu_2^2 u^2/v^2)^{\frac{1}{2}}} \right), \quad . \quad . \quad (39)$$

from which $H = -dA/dh$ gives

$$H = qu \left(1 - \frac{u^2}{v^2} \right) \left[\frac{\nu_1}{r_1 (1 - \nu_1^2 u^2/v^2) + r_1 \mu_1 (1 - \nu_1^2 u^2/v^2)^{\frac{1}{2}}} \right. \\ \left. - \text{same function of } r_2, \mu_2, \nu_2 \right], \quad . \quad (40)$$

where $\mu = \cos \theta$, $\nu = \sin \theta$.

When P is vertically over B , and A is at an infinite distance, we shall find

$$H = qu/h, \quad . \quad . \quad . \quad . \quad . \quad (41)$$

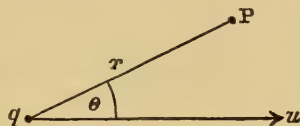
which is one half the value due to an infinitely long (both ways) straight current of strength qu . The notable thing is the independence of the ratio u/v .

But if $u=v$ in (40), the result is zero, unless $v_1=1$, when we have the result (41). But if P be still further to the left, we shall have to add to (41) the solution due to the electrification which is ahead of P. So when the line is infinitely long both ways, we have double the result in (41), with independence of u/v again.

But should q be a function of z , we do not have independence of u/v except in the already considered case of $u=v$, with plane waves, and no component of electric force parallel to the line of motion.

17. Next, let the electrified line be in steady motion perpendicularly to its length.

Let q be the linear density (constant), the z -axis that of the motion, the x -axis coincident with the electrified line and that of y upward on the paper. Then the A at P will be



$$A = \frac{qu}{(1-u^2/v^2)^{\frac{1}{2}}} \log \frac{x_1 + \{x_1^2 + y^2 + z^2(1-u^2/v^2)^{-1}\}^{\frac{1}{2}}}{x_2 + \{x_2^2 + y^2 + z^2(1-u^2/v^2)^{-1}\}^{\frac{1}{2}}}; \quad (42)$$

where y and z belong to P, and x_1, x_2 are the limiting values of x in the charged line. From this derive the solution in the case of an infinitely long line. It is

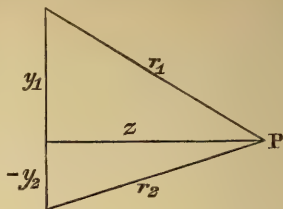
$$cE = \frac{2q}{r} \cdot \frac{(1-u^2/v^2)^{\frac{1}{2}}}{1-v^2u^2/v^2}, \quad H = cEuv, \quad . \quad . \quad . \quad (43)$$

where $v = \sin \theta$; understanding that \mathbf{E} is radial, or along qP in the figure, and \mathbf{H} rectilinear, parallel to the charged line.

Terminating the fields internally at $r=a$, we have the case of a perfectly conducting cylinder of radius a , charged with q per unit of length, moving transversely. When $u=v$ there is disappearance of \mathbf{E} and \mathbf{H} everywhere except in the plane $\theta = \frac{1}{2}\pi$, as in the case of the sphere, and consequent infinite values. It is the curvature that permits this to occur, *i. e.* producing infinite values; of course it is the self-induction that is the cause of the conversion to a plane wave, here and in the other cases. There is some similarity between (43) and (29). In fact, (43) is the bidimensional equivalent of (29).

18. Coming next to a plane distribution of electrification, *Phil. Mag. S. 5. Vol. 27. No. 167. April 1889.* Z

let q be the surface density, and the plane be moving perpendicularly to itself. Let it be of finite breadth and of infinite length, so that we may calculate H from (43). The result at P is



$$H = \frac{qu}{(1-u^2/v^2)^{\frac{1}{2}}} \log \frac{r_1^2 - y_1^2 u^2/v^2}{r_2^2 - y_2^2 u^2/v^2} \quad \dots \quad (44)$$

When P is equidistant from the edges, H is zero. There is therefore no H anywhere due to the motion of an infinitely large uniformly charged plane perpendicularly to itself. The displacement-current is the negative of the convection-current and at the same place, viz. the moving plane, so there is no true current.

Calculating E_1 , the z -component of E , z being measured from left to right, we find

$$cE_1 = 2q \left\{ \tan^{-1} \frac{y_1}{z} \left(1 - \frac{u^2}{v^2}\right)^{\frac{1}{2}} - \tan^{-1} \frac{y_2}{z} \left(1 - \frac{u^2}{v^2}\right)^{\frac{1}{2}} \right\} \quad (45)$$

The component parallel to the plane is H/cu . Thus, when the plane is infinite, this component vanishes with H , and we are left with

$$cE_1 = cE = 2\pi q, \quad \dots \quad (46)$$

the same as if the plane were at rest.

19. Lastly, let the charged plane be moving in its own plane. Refer to the first figure, in which let AB now be the trace of the plane when of finite breadth. We shall find that

$$H = 2qu \left[\tan^{-1} \frac{z}{h(1-u^2/v^2)^{\frac{1}{2}}} \right]_{z_1}^{z_2} \quad \dots \quad (47)$$

z_1 and z_2 being the extreme values of z , which is measured parallel to the breadth of the plane.

Therefore, when the plane extends infinitely both ways, we have

$$H = 2\pi qu \quad \dots \quad (48)$$

above the plane, and its negative below it. This differs from the previous case of vanishing displacement-current. There is H , and the convection-current is not now cancelled by co-existent displacement-current.

The existence of displacement-current, or changing displacement, was the basis of the conclusion that moving electrification constitutes a part of the true current. Now in the

problem (48) the displacement-current has gone, so that the existence of H appears to rest merely upon the assumption that moving electrification is true current. But if the plane be not infinite, though large, we shall have (48) nearly true near it, and away from the edges; whilst the displacement-current will be strong near the edges and almost nil where (48) is nearly true.

But in some cases of rotating electrification, there need be no displacement anywhere, except during the setting up of the final state. This brings us to the rather curious question whether there is any difference between the magnetic field of a convection-current produced by the rotation of electrification upon a good nonconductor and upon a good conductor respectively, other than that due to diffusion in the conductor. For in the case of a perfect conductor, it is easy to imagine that the electrification could be at rest, and the moved conductor merely slip past it. Perhaps Professor Rowland's forthcoming experiments on convection-currents may cast some light upon this matter.

December 27, 1888.

XL. The Rotation of the Plane of Polarization of Light by the Discharge of a Leyden Jar. By Dr. OLIVER LODGE.*

THE current produced by the discharge of a Leyden jar is so violent while it lasts, that those phenomena which depend upon the value of a current independently of its duration are well excited by it. Such are the induction of currents, the production of magnetism, and the rotation of the plane of polarization.

Nothing is easier than to wind a quantity of thin gutta-percha-covered wire round a piece of heavy glass, and to witness the bright flashing of a dark field between polarizer and analyser whenever a large Leyden jar is sparked through the coil, the source of light being a paraffin-lamp or gas-flame. The suddenness of the effect suggests, of course erroneously, that it is an illumination caused by the light of the spark which one is looking at.

The fact that the discharge is oscillatory, and that the restoration of light in the dark field is oscillatory too, is proved by the fact that an adjustment of the analyser to one side or the other of complete darkness has just the same effect on the result. It is proved also by the fact that a biquartz

* Communicated by the Physical Society: read March 9, 1889.

exhibits no change in its sensitive tint during the discharge ; similarly also Jellett, and other such reversible detectors, would be useless for the purpose : the effect is an irreversible one. Nevertheless some sort of measure of the effect can be made by finding the position of the analyser whereat the brightness of the field suffers no appreciable change at the occurrence of the spark, because in one position the oscillation on one side will darken it just as much as that on the other side brightens it.

It seemed to me possible that if the effect could be rendered pretty considerable a slight *darkening* of the field might be obtained with some adjustments, because some arcs of the sine curve have an average ordinate less than their middle ordinate. But when a fairly bright field winks slightly it is not easy to say whether it winks brighter or darker, and after all I do not know that it much matters.

The main interest in the experiment seemed to me to lie in the evidence it afforded of practical instantaneity in the development of the property in the substance under examination ; and in order to try oscillations of much greater frequency than those I first used, I got my assistant, Mr. Robinson, to make a long tube of carbon disulphide with which to repeat the experiment in a sensitive manner.

Now, a most interesting experiment of Villari*, in which he whirled a drum of heavy glass up to 200 revolutions per second between the poles of a magnet, and perceived the electro-optic effect to diminish from 100 revolutions per second upwards, and ultimately nearly cease at high speeds (say 180 per second), had led one to suppose that some distinct time was necessary for the production of the effect—something between $\frac{1}{800}$ and $\frac{1}{400}$ second. But on referring to that most useful summary of Electrical Science, Prof. Chrystal's article in the *Encycl. Brit.*, I found, along with a quotation of Villari's experiment, a statement that Professors Bichat and Blondlot†, of Nancy, had by means of Leyden-jar discharge proved that practically no time was necessary. I accordingly procured a copy of the volume of the *Comptes Rendus* from London, and there found an all too brief account of a most beautiful series of experiments, by which they considered it proved that if any time is required, it is less than the $\frac{1}{30,000}$ second. The skill of these French Professors in optics, and their previous researches in connexion with the Faraday

* Pogg. *Ann.* cxlix. 1873, p. 324. Translated from the *Rendiconti Istit. Lombardo*, ser. 2, vol. iii.

† *Comptes Rendus*, xciv. 1882, p. 1590.

effect in various substances, are well known; and what I have to show to-day is practically nothing more than a repetition of their experiments with the theory worked out.

I take either a bit of heavy glass in its helix, or, for projection, a tube of CS_2 a yard long surrounded by four large helices, each containing about 80 yards of gutta-percha-covered No. 16 wire; and on passing the discharge from a battery of several jars the field flashes out bright, in what may be (if one is looking direct towards the hot lime) quite a dazzling manner.

I have just received a post-card from M. Bichat, in answer to an inquiry, saying that the coil they used was the secondary bobbin of a Rühmkorff coil with a resistance of 5000 ohms. Now, whether this was by accident or by design, it was difficult for them to use a coil more suited to the purpose, or one that would give a larger effect, as I shall show directly. The Leyden jars employed by them were either one or two, about 18 inches high and 6 inches in diameter. We shall find that the effect increases in direct proportion to the capacity of the jars.

To find out whether any time was required for the development of the effect, they made the light coming through the tube illuminate a slit, the spark being made to illuminate another slit close above the first, and then both slits were examined in a rotating mirror. Both were spread out into a discontinuous band, and the serrations of the one agreed as nearly as could be seen with the serrations of the other. Thus proving in a beautiful manner that there was practically no lag of the effect behind its cause, and thereby contradicting the conclusion of Villari*.

Meanwhile I had been doing similar experiments, but with a bobbin of much smaller inductance, and using a still smaller capacity, my object being to find the greatest frequency able to show the effect distinctly. If, for instance, heavy glass or CS_2 was able to follow oscillations of some million per second, there could be no further question but that Villari's conclusion was wrong. I find that the CS_2 is able to show the effect when the rate of alternation is 70,000 per second; and though the short length of heavy glass available does not enable me at present to make quite the same statement for it, I have no

* They also mention an unpublished experiment by MM. Curie and Ledeboer, in which a disk of glass was spun between the poles after the manner of a Foucault disk of copper, so that the path of the light was parallel to the axis of rotation, instead of perpendicular to it as in Villari's drum arrangement, and in that case no diminution was observed. If this experiment is yet published I am ignorant of it.

reason to suppose it in any way inferior. In fact, experiment distinctly suggests that the effect is practically instantaneous ; but as to the degree of instantaneity I shall be able to make a more exact numerical statement later on.

It may be of interest to the Physical Society to have the oscillatory character of the restored light demonstrated, and there is no difficulty in the experiment. One sets the analyser to as near darkness as possible, one receives the trace of residual light upon a rotating mirror, by which it is spread out into a faint band ; and on then sending sparks through the coil of wire round the tube of CS_2 , the band brightens and presents a distinctly beaded appearance at every spark.

Rotating the analyser a little, every alternate bead grows fainter, while the other alternate ones brighten, thus proving most directly the oscillatory character of the light and of the Leyden-jar discharge.

Still more feasible is it to spread out the light of the spark itself into a serrated band, but in making this visible to an audience it is well to save time by exciting the jars with a large induction-coil instead of a Wimshurst machine, because not only can sparks be thus got much more frequently, but each spark is multiple—the jars filling and overflowing several times during the one coil-discharge. The multiple or intermittent spark is analysed by the revolving mirror into a number of serrated bands one after the other, and while one band is in the field of view of one part of the audience another band may be visible to another.

Although when a suitable circuit is employed the analysis of the spark in a mirror such as is used for manometric flames rotating not more than three or four times a second is easy, yet with ordinary discharging circuits I have used small mirrors spinning 200 times a second and failed to see any certain trace of oscillation ; while, as is well known, Wheatstone used mirrors rotating 800 times a second, and got the image-spark only barely elongated : not in the least serrated. It may be worth while, therefore, to state the kind of circuit which I have recently employed. The capacity consists of a couple of condensers built up in the laboratory with double thicknesses of window-glass alternating with tinfoil, and the whole flooded in paraffin till it is a solid mass in a box of teak weighing a couple of hundredweight. These condensers stand a considerable length of spark, 3 or 4 inches for instance, but their strength is not in the least called out in the experiments now related. Their capacity when joined up parallel is .048 microfarad. These are often supplemented by

a battery of ordinary Leyden jars and by single jars, which altogether raise the capacity to $\cdot 074$ microfarad or 670 metres. To vary the capacity in the ratio 1, 2, 4, I use a switch consisting of 6 glass pillars, each with a bit of close-fitting glass tube cemented on the top to serve as a mercury cup. The leading wires are held in the mercury cups by indiarubber umbrella-rings round the glass pillars; and insulated wire bridges easily make connexion between one pillar and the next.

In the annexed figure dotted lines indicate sufficiently the permanent connexions. M being machine and discharger, L being circuit and coils. The movable connexions are made in pairs as follows:—

For series or cascade, connect 2, 3; 4, 5.

For one condenser only, connect 3, 4.

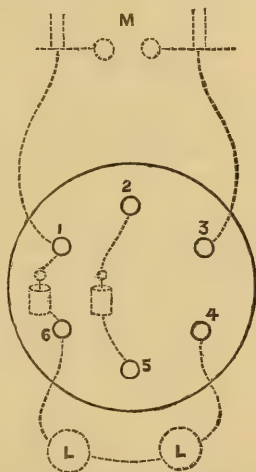
For both condensers in parallel, connect 1, 2; 3, 4; 5, 6.

As regards circuit, one of the coils I use is a hank of 440 yards of thickly covered No. 16 G. P. wire as it came from the maker, with a self-induction of $\cdot 048$ secohm and a resistance of 3.75 ohms.

The coils on the CS_2 tube consist of the same kind of wire; they have a resistance rather greater than the above, and a combined self-induction of about $\cdot 008$ secohm.

A number of gigantic electric-light cables have been inserted in the circuit at different times, one on a bobbin like two cart-wheels being kindly lent by the Engineers of the Liverpool Electric Supply Company; but the easiest way of getting very large self-induction is to use No. 20 or No. 22 G. P. or I. R. wire not too thickly covered. I have recently obtained a large coil wound so as to give maximum self-induction which I estimate as 7 or 8 secohms; I have not yet used this, but I frequently use a number of coils of No. 21 wire packed together as close as is easily possible, and making a total self-induction of 1 secohm or ten thousand kilometres.

Without going into further details, I may say that the observed frequency of the oscillations, as estimated from the appearance of the serrations in the revolving mirror, or by the pitch of the musical note accompanying the spark, agrees



very respectably with the frequency as calculated from

$$\frac{3 \times 10^{10}}{2\pi \sqrt{\left(\frac{L}{\mu} \cdot \frac{S}{K}\right)}}$$

Returning now to the magneto-optic effect of the Leyden-jar discharge, I make out its theory to be as follows:—

The current at any instant during the discharge is

$$C = \frac{V_0}{pL} e^{-mt} \sin pt \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\text{where } m = \frac{R}{2L} \text{ and where } m^2 + p^2 = \frac{1}{SL}.$$

The difference of magnetic potential between the two ends of a rod of length l surrounded by a very long solenoid of n_1 turns of wire on unit length of it is

$$4\pi\mu C n_1 l.$$

The Verdet constant for carbon disulphide, or the rotation of the plane of polarization per unit difference of magnetic potential is, according to the determination of Lord Rayleigh, .04202 minute for a temperature of 18° Centigrade, and for sodium light. Calling this k , we have the rotation effected by the current

$$\theta = 4\pi k n_1 l \mu C \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the case of the heavy glass experiment a helix longer than the stalk of glass is employed; but in the case of the long tube of CS_2 and four helices there will be some correction necessary for the ends of the helices. This correction is, however, well understood, and there is no special need to introduce it at present.

Now the total amplitude of the light falling upon the analyser being a (this amplitude being all stopped if the analyser is set to darkness, and all transmitted if it be turned 90°), the component amplitude which will at any instant get through the analyser set to darkness, when the plane of polarization has been rotated through an angle θ , is

$$a \sin \theta;$$

and the amount of light which gets through during the time τ during which an impression is capable of being accumulated on the retina, *i.e.* before it has begun to die away as fast as it is produced, is

$$\int_0^\tau (a \sin \theta)^2 dt.$$

The amount of light which would get through in the same time if the analyser were set to maximum brightness would be $\alpha^2\tau$.

These expressions, then, furnish the measure of the respective effects upon the retina; and so we have

$$\frac{\text{apparent brightness restored by the spark}}{\text{maximum brightness possible}} = \frac{\int_0^\tau \alpha^2 \sin^2 \theta \, dt}{\alpha^2 \tau}.$$

Now inasmuch as τ is comparable to the time of persistence of retinal impression, being perhaps equal to it, and since this time is greatly longer than any ordinary duration of a spark-discharge, the above ratio of the relative brightnesses reduces, for practical purposes, to

$$B = \text{relative brightness} = \frac{1}{\tau} \int_0^\infty \sin^2 \theta \, dt, \quad \dots (3)$$

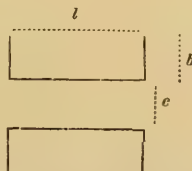
and this is what the eye will observe.

Referring back to equations (1) and (2) we have the means of determining this quantity. I do not know how to do it completely, but for the case when θ is moderately small the integral is easy, viz. :—

$$\begin{aligned} B &= 16\pi^2 k^2 \mu^2 n^2 \frac{V_0^2}{p^2 L^2 \tau} \int_0^\infty e^{-2mt} \sin^2 pt \, dt \\ &= 160 k^2 \mu^2 n^2 \frac{\frac{1}{2} S V_0^2}{R \tau}. \quad \dots \dots \dots (4) \end{aligned}$$

The effect thus depends directly on the square of the total number of turns of wire employed, directly on the energy of the static charge used, and inversely on the resistance of the circuit.

To find the best size of wire to wind on a bobbin of given size, for the purpose, one can write down the value of n^2/R ; given the length of the bobbin as l , its depth of winding-space b , the diameter of its empty core c . Call the radius of the uncovered wire used ρ , and its radius when covered ρ' .



First, supposing no appreciable resistance in the rest of the circuit, it comes out

$$\frac{n^2}{R} = \frac{lb}{4(c+b)} \left(\frac{\rho}{\rho'} \right)^2, \quad \dots \dots \dots (5)$$

which means that the size of the wire does not matter, but that it is important to keep the covering thin. Only then

unfortunately the discharge is liable to burst the covering. In my helices $\rho' = 3\rho$, which is rather excessive, because it reduces the visible effect to one ninth of what it might be if no covering were used.

It is impossible to press this to its natural consequence of using only one turn of thick sheet copper, because the resistance of the rest of the circuit has been neglected. Taking it into account, as r , the relation becomes

$$\frac{n^2}{R} = \frac{\frac{lb}{4(c+b)} \cdot \left(\frac{\rho}{\rho'}\right)^2}{1 + \frac{4r}{lb(c+b)} (\rho \rho')^2}, \quad . \quad . \quad . \quad (6)$$

which contains the product of the sizes of covered and uncovered wire as well as the ratio; and this product occurs in the term containing r , the external resistance. Hence, to keep this term small, it is desirable to use wire thin enough to throw the major part of the resistance into the bobbin; and there is no limit to the thinness of the wire that may be advantageously employed, until the thickness of the covering bears too high a ratio to the whole. And inasmuch as insulation thickness may be more judiciously distributed between layers than between consecutive turns, it is obvious how extremely suitable a coil for the purpose is the secondary of a Rühmkorff.

I said that I did not know how to evaluate the complete integral involved in (3) when θ is not small; but, as usual, I sent the problem to my brother, and he speedily reduced it to a form equivalent to this:—

$$B = \frac{L}{R\tau} \int_0^A \frac{1 - J_0(x)}{x} \cdot dx, \quad . \quad . \quad . \quad (7)$$

where

$$A = 8\pi kn\mu V_0 \sqrt{\left(\frac{S}{L}\right)}.$$

I have asked him to write a short appendix to this paper.

Returning now to consider the meaning of these equations, and attempting a numerical estimate of what to expect in practice, we shall find that though the instantaneous rotation expressed by (2) is enormous, being quite possibly 60 or even 180 degrees, yet the restoration of light expressed by (4) is but feeble, and only some ten thousandth part or so of what could be gained by rotating the Nicol. This explains why it is fairly easy to analyse the restored light into a beaded

band, because then one gets the effect of the separate oscillations ; and they are very bright though very momentary.

Looking at the formula (4), it is clear that to get large effects it is desirable to use a large battery of jars, and to charge to a high potential, only that one is afraid of breaking down the coil. Large capacity and a great number of turns of wire are the safest ways of increasing the effects.

One sees also that extra self-induction in the circuit does neither good nor harm to the resultant effect. It diminishes the effect of each oscillation, but it prolongs the time during which they last. And it is the total "*action*" of the series of decaying swings which the eye perceives. One sees that extra resistance in the circuit is wholly bad.

As to the cause of what must now be regarded provisionally as the erroneous conclusion of Villari I see no reason to doubt his experiments, though he does not give sufficient details to enable one to arrive at a perfectly satisfactory judgment on all points ; it is practically certain that he did get a much diminished effect on spinning the flint-glass drum between the poles of the magnet and sending the light along successive diameters of the drum.

But the cause of this I venture to suggest is possibly to be found in the state of strain into which the glass will be thrown by centrifugal force. It may be said that, if so, the light ought to have been similarly affected even when no magnetic field was employed ; and Villari expressly says that this was not the case.

But then it is to be noticed that, when no magnetic rotation is attempted, the aspect of the plane of polarization to the stress remains constant throughout the journey ; and if light happens to enter with no component modifiable by the stress, it will go out in the same condition. Whereas when rotation has taken place inside the glass this constancy of aspect is destroyed, and the light on exit has a different component modifiable by the strain to what it had on entrance.

I do not profess to be able to give a coherent account of how this cause shall give rise to a reduction in the rotation instead of to an elliptical polarization. But then neither am I able to extract from Villari's account of his experiments any assurance that some elliptic polarization was not produced, and that the reduction of the rotation of the polarized plane was anything more than a mixture of small effects not easily analysable nor precisely defined.

It is in any case a most interesting experiment, and should be repeated so as to really get at the bottom of the cause of the observed phenomenon. There are many other experiments on whirling glass which may likewise be made.

APPENDIX, by ALFRED LODGE, M.A., *Coopers Hill, Staines.*

The value of

$$\int_0^\infty \sin^2 \theta \, dt, \text{ where } \theta = Ae^{-mt} \sin nt, \text{ and } C = \frac{An}{\sqrt{m^2 + n^2}},$$

is $\frac{1}{2m} \left(\frac{1}{2} \cdot C^2 - \frac{1}{4} \cdot \frac{C^4}{(\frac{1}{2})^2} + \frac{1}{6} \cdot \frac{C^6}{(\frac{1}{3})^2} - \frac{1}{8} \cdot \frac{C^8}{(\frac{1}{4})^2} + \dots \right).$

For

$$\begin{aligned} \int_0^\infty \sin^2 \theta \, dt &= \frac{1}{2} \int_0^\infty (1 - \cos 2\theta) \, dt \\ &= \frac{1}{2} \int_0^\infty \left(\frac{(2\theta)^2}{2} - \frac{(2\theta)^4}{4} + \dots \right) dt. \end{aligned}$$

The general term of this series is

$$\frac{2^{2p} A^{2p}}{2p} \int_0^\infty e^{-2mpt} \sin^{2p} nt \, dt,$$

and can be integrated by successive reduction.

Let u_{2p} denote

$$\int_0^\infty e^{-2mpt} \sin^{2p} nt \, dt;$$

then, integrating by parts,

$$\begin{aligned} u_{2p} &= \left[\frac{-e^{-2mpt}}{2mp} \cdot \sin^{2p} nt \right]_0^\infty + \int_0^\infty \frac{e^{-2mpt}}{2mp} \cdot 2np \sin^{2p-1} nt \cdot \cos nt \, dt \\ &= 0 + \frac{n}{m} \int_0^\infty e^{-2mpt} \sin^{2p-1} nt \cdot \cos nt \, dt; \end{aligned}$$

and again integrating by parts,

$$\begin{aligned} &= -\frac{n}{m} \frac{e^{-2mpt}}{2mp} \cdot \sin^{2p-1} nt \cdot \cos nt \Big|_0^\infty \\ &\quad + \frac{n}{m} \int_0^\infty \frac{e^{-2mpt}}{2mp} \left((2p-1) \sin^{2p-2} nt \cos^2 nt - \sin^{2p} nt \right) \cdot n \cdot dt \\ &= 0 + \frac{n^2}{2m^2 p} \int_0^\infty e^{-2mpt} \left((2p-1) \sin^{2p-2} nt - 2p \cdot \sin^{2p} nt \right) dt \\ &= \frac{n^2}{m^2} \cdot \frac{2p-1}{2p} \cdot u_{2p-2} - \frac{n^2}{m^2} \cdot u_{2p}. \end{aligned}$$

$$\therefore \left(1 + \frac{n^2}{m^2}\right) u_{2p} = \frac{n^2}{m^2} \cdot \frac{2p-1}{2p} u_{2p-2};$$

$$\therefore u_{2p} = \frac{n^2}{m^2 + n^2} \cdot \frac{2p-1}{2p} \cdot u_{2p-2}$$

$$= \left[\frac{n^2}{m^2 + n^2} \right]^p \cdot \frac{2p-1}{2p} \cdot \frac{2p-3}{2p-2} \dots \frac{3}{4} \cdot \frac{1}{2} \int_0^\infty e^{-2mpt} dt$$

$$= \frac{1}{2mp} \left[\frac{n^2}{m^2 + n^2} \right]^p \cdot \frac{(2p-1)(2p-3) \dots 3 \cdot 1}{2p(2p-2) \dots 4 \cdot 2} = \frac{|2p|}{2^{2p} |p| |p|};$$

$$\therefore \frac{(2A)^{2p}}{|2p|} u_{2p} = \frac{A^{2p}}{2mp(|p|)^2} \cdot \left[\frac{n^2}{m^2 + n^2} \right]^p = \frac{C^{2p}}{2mp(|p|)^2};$$

$$\therefore \int_0^\infty \sin^2 \theta dt = \frac{1}{2m} \left(\frac{1}{2} \cdot C^2 - \frac{1}{4} \cdot \frac{C^4}{(|2|)^2} + \frac{1}{6} \cdot \frac{C^6}{(|3|)^2} - \dots \right).$$

Q. E. D.

N.B.—The symbols used for the constants throughout this Appendix have no connexion with those in the main paper, except that m is the same in both. The value of the C of the Appendix, expressed in terms of the symbols used in the paper, is

$$4\pi kn\mu V_0 \sqrt{S \div L}.$$

XLI. *On the Use of Lissajous' Figures to determine a Rate of Rotation, and of a Morse Receiver to Measure the Periodic Time of a Reed or Tuning-fork.* By Prof. J. VIRIAMU JONES, M.A.*

IT is sometimes of importance to determine with great accuracy the angular velocity of a rotating body at a given instant. For instance, in measuring an electrical resistance in absolute measure by the British Association method or the method of Lorenz, the rate of rotation of the revolving coil or disk must be known with full accuracy at the time when the reading of a galvanometer-needle is taken.

The method I have to bring before the Physical Society in this note consists in obtaining, by means of Lissajous' figures, equality of period between the rotating body and a reed maintained in vibration electrically, and then subsequently determining the vibration-period of the reed.

* Communicated by the Physical Society: read March 23, 1889.

(i.) *The use of Lissajous' Figures to obtain Equality of Period between the Revolving Apparatus and the Reed.*

In one end of the axle of the revolving apparatus (which in the application of this method in the laboratory of the University College, Cardiff, is a Lorenz disk rotating about a horizontal axis) a pin is placed excentrically. The pin fits accurately in a hole in a rod free at one end, and constrained to move only longitudinally at the other.

Fig. 1 represents an end view of the axle M M M. The excentric pin is indicated by the dotted circle at B. The pin fits into a hole in the rod S S free at the end C, the other end, A, being constrained to move only longitudinally.

When the apparatus is in rotation the excentric pin impresses on the rod a vibratory motion; and the vibration-period of the rod is equal to the revolution-period of the revolving disk.

To the free end of the rod a mirror, T, is attached.

In the apparatus in my laboratory, already mentioned, the axle is horizontal and the rod moves in a vertical plane.

The reed is a steel bar 100 centim. long, and its section is rectangular, 1.51 centim. by .60 centim. It is clamped in a massive iron frame, and by moving the bar in the frame the vibrating segment may be made longer or shorter so as to vary its vibration-period. The period may be further adjusted by the movement of a rider.

Fig. 2 is a plan of the reed and its electrical arrangements, which are a little different to those in ordinary use in order to allow, without inconvenient readjustment of the springs, the lengthening or shortening of the vibrating segment.

A A A is the steel bar.

C C the iron frame to which it is clamped by the bolts and nuts D, D. This stand is clamped to a large wooden block resting on a concrete floor.

F F F is a wooden piece attached at N N to the iron frame, and bearing the electromagnet M.

On the opposite side of the vibrating bar to the electromagnet there is a pair of springs S_1 . One of the pair is always in contact with the bar as it vibrates, and the other, fitted with screw-adjustment, makes and breaks contact with the first. The first spring is connected through the electromagnet with the pole of a suitable battery, the other pole being connected with the second spring. The apparatus gives no trouble, and once started the reed will vibrate for hours without attention.

To the free end of the vibrating segment a mirror T T is

Fig. 1.—Vibrating Mirror on Disk. Full size.

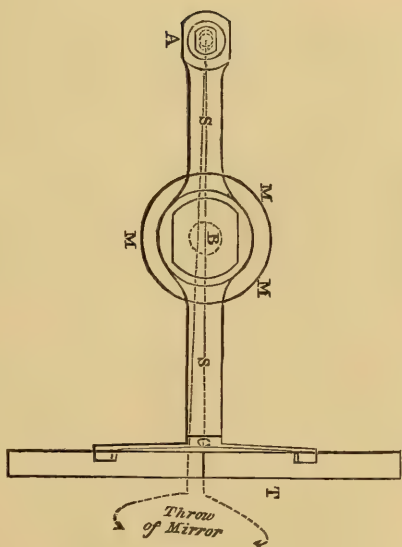
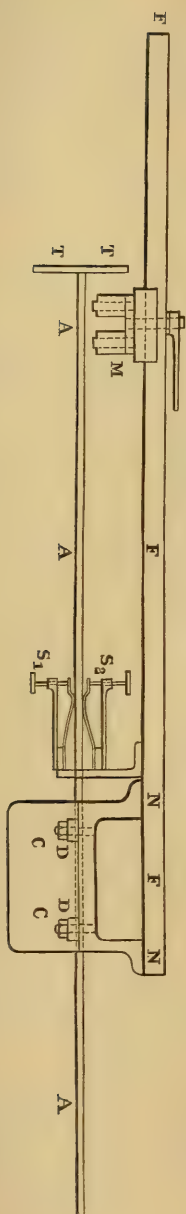


Fig. 2.—Reed and Stand, $\frac{1}{4}$ full size.



attached. In my apparatus the reed moves in a horizontal plane, and so its plane is at right angles to that of the vibrating rod attached to the axle of the rotating disk. Therefore the image of a spot of light seen by reflexion in both mirrors (viz. that attached to the rod on the axle, and that attached to the reed) will for synchronism be drawn out into some form of ellipse, and the permanence of the ellipse is an exact test of equality between the vibration-period of the reed and the period of revolution of the rotating-apparatus.

(ii.) *Determination of the Vibration-period of the Reed by a Morse Receiver.*

Opposite the pair of springs S_1 connected with the electro-magnet M and concerned with the maintenance of the reed in vibration, there is another pair of springs S_2 , similar in every respect. One of these is connected through a Morse receiver with a battery of the requisite electromotive force, the other being connected with the other pole of the battery. When the reed is in vibration it makes and breaks contact between this pair of springs; and we have, therefore, on the tape of the Morse instrument a series of dashes; a dash and the blank space between it and the next corresponding to one vibration of the reed.

On the same tape, side by side with the track of the inking-wheel of the Morse instrument, a pen is made to record the movement of the pendulum of the standard clock in a sufficiently ordinary manner. We have then merely to count the number of dashes in a given time, and we have the number of vibrations in that interval, and hence the pitch of the reed.

The electrical connexion is made in my laboratory by the escapement-wheel of the clock; and hence for accurate result the time taken must be an integral multiple of a minute, the period of revolution of the wheel.

This method of determining the vibration-period of a reed or tuning-fork has seemed to me to be of extreme simplicity, and by taking a sufficient interval of time may be made of any desired accuracy. A Morse receiver will spin out its tape at a speed of 40 feet a minute, and the ordinary rolls of tape will last for at least 15 minutes when it is doing so.

The limit of accuracy in this method of determining the rate of vibration is, I find at present, the constancy of vibration of the reed. It has been a surprise to me to find that the vibration-period is not constant to much more than 1 part in 1000.

In conclusion, I desire to express my thanks to Mr. John Gavey, of the S. Wales Telegraphic Department, for the help he has rendered me in connexion with this matter.

University College, Cardiff,
March 22nd, 1889.

XLII. On the Loss of Voltaic Energy of Electrolytes by Chemical Union. By Dr. G. GORE, F.R.S.*

IN a research on the "Influence of the Chemical Energy of Electrolytes upon the Change of Potential of a Voltaic Couple in Water" (Proc. Roy. Soc. 1888, vol. xlv. pp. 300-308) I have shown that the minimum proportions of various substances required to change the potential of a voltaic cell of un-amalgamated zinc, platinum, and distilled water, when balanced by a perfectly similar cell, and to visibly move the needles of an ordinary astatic torsion galvanometer of 100 ohms' resistance (see sketch) were as follows :—

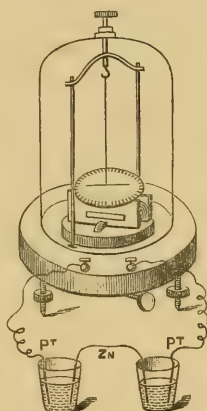


TABLE I.

Minimum Proportion.

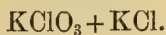
	Between 1 part in	Parts of Water	Average. 1 part in
Chlorine..	1,264,000,000 and	1,300,000,000	1,282,000,000
Bromine..	77,500,000	84,545,000	81,022,500
Iodine ..	3,100,000	3,521,970	3,310,985
KCl	695,067	704,540	699,803
KBr	66,428	67,391	66,909
KI	15,500	17,222	16,361
KIO ₃	443	494	468
KBrO ₃ ..	344	384	364
KClO ₃ ..	221	258	239

* From the Proceedings of the Birmingham Philosophical Society, vol. vi. part 2. Communicated by the Author.

These numbers represent the relative amounts of voltaic energy exerted with zinc, by one part by weight of each of the substances, when added to one of the portions of water, and show that the electro-negative energy (or power of exciting a voltaic couple) of the halogens chlorine, bromine, and iodine is very greatly reduced by their chemical union with potassium, and that of the resulting compound is still further reduced by additional union with oxygen.

In order to ascertain whether the energy was diminished by every additional different substance added to and chemically united with the original substance, or with the subsequently formed group of bodies, I made the following measurements with substances in the proportions of their molecular weights. The chemical compounds formed may be recognized by having the smallest amounts of voltaic energy (see "A Method of Detecting Dissolved Chemical Compounds and their Combining Proportions," Proc. Roy. Soc. January 19th, 1889), and are indicated by a \times . Distilled water was employed in making all the solutions, and the substances used were sufficiently pure for the purpose.

TABLE II.



	Between 1 part in	Parts of Water.	Temp.	Average. 1 part in
KCl	695,067 and	704,540 at	12° C.	699,803
4KClO ₃ + 5KCl	56	63	20	59.5
4 „ + 4 „ \times ..	50	56	20	53
4 „ + 3 „	55	63	20	59
„	221	258	10	239

In this case the mean relative amount of voltaic energy of the original substances averaged 350,021, and was reduced to 53 by the formation of a definite chemical compound having the formula KClO₃, KCl.

In order to reduce the voltaic energy still further I used the following substances :—

TABLE III.

KClO₃, KCl + Oxalate of Ammonium.

	Between 1 part in	and	Parts of Water	Temp.	1 part in
Am ₂ C ₂ O ₄	77		86	at 19° C.	81·5
3KClO ₃ , KCl + 4Am ₂ C ₂ O ₄ ..	31·6		35·2	11	33·4
4 „ „ +4 „ ×	26·7		30	11	28·4
5 „ „ +4 „ ..	28·6		32·2	11	30·4
8 „ „ +4 „ ..	33		37	11	35
„ „	50		56	20	53

In this case the mean relative amount of energy was 67·2, and was reduced to 28·4 by the formation of a definite chemical compound having the formula KClO₃, KCl, Am₂C₂O₄.

In the experiments of these three tables, 1st, by the chemical union of 1 atomic weight of chlorine with its chemical equivalent of potassium to form KCl, the electro-negative energy was reduced from 1282 millions to 699,803 ; 2nd, by the union of 1 molecular weight proportion of KCl with oxygen, the energy was further reduced to 239 ; 3rd, by the union of 1 molecular proportion of KClO₃ with its equivalent of KCl, the energy was still further diminished to 53 ; and 4th, by the union of KClO₃, KCl with its equivalent or 1 molecular weight proportion of ammonium oxalate, the energy was finally decreased to 28·4. The total reduction was from 1282 millions to 28·4.

The following are additional examples of reduction of voltaic energy attending increased complexity of chemical composition :—

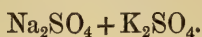
TABLE IV.

NaCl + KCl.

	Between 1 part in	and	Parts of Water	Temp.	1 part in
KCl	695,067		704,540	at 12° C.	699,803
NaCl + 2KCl	12,443		13,840	20	13,141
4 „ +5 „	8,611		9,687	20	9,149
4 „ +4 „ ×	5,548		6,370	20	5,959
5 „ +4 „	7,154		8,017	19	7,585
„	193,750		221,428	15	207,589

In this case the mean relative amount of voltaic energy of the original substances was 453,696, which was reduced to 5959 by the formation of a chemical compound represented by the formula NaCl, KCl.

TABLE V.

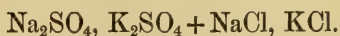


	Between 1 part in		Parts of Water	Temp.	1 part in
K_2SO_4	2,152	and	2,396	at 12°C .	2,274
$2\text{Na}_2\text{SO}_4 + 3\text{K}_2\text{SO}_4$	137		150	17	143
2 „ +2 „ × ..	106		119	17	112
3 „ +2 „	145		160	17	152
„	1,914		2,126	13	2,020

The mean energy of the original substances was 2147, and was decreased to 112 by the formation of a compound having the formula Na_2SO_4 , K_2SO_4 .

In the next table the reduction of energy is carried further :—

TABLE VI.



	Between 1 part in		Parts of Water	Temp.	1 part in
NaCl, KCl	5,548	and	6,370	at 20°C .	5,959
$2\text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4 + 5\text{NaCl}, \text{KCl}$.	44		47	16	45.5
2 „ „ +4 „ „ × ..	39		43	16	41
2 „ „ +3 „ „	50		55	16	52.5
„ „	106		119	17	112

In this case the mean relative energy of the constituents was 3085, and was reduced to 41 by the formation of a definite compound having the formula Na_2SO_4 , K_2SO_4 , 2NaCl , KCl.

In Table VII. the energy is so much more diminished that it has become a negative quantity, *i. e.* it is *less* than that with water alone, the change of potential on adding the substance being a decrease of electromotive force instead of an increase :—

TABLE VII.

 $\text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl}, \text{KCl} + \text{K}_2\text{CrO}_4.$

	Between 1 part in	Parts of Water	Temp.	1 part in
$\text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl} \text{ KCl}$	39	and 43	at $16^\circ \text{C}.$	41
$5\text{K}_2\text{CrO}_4 + \text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4,$ $2\text{NaCl}, \text{KCl} \dots\dots\dots$	-129	-143	14	-136
4 „ + „ „ ×	-212	-238	14	-225
3 „ + „ „ ..	-115	-129	14	-122
„	5.3	7.04	13	6.2

The mean average energy of the substances was decreased from 20.6 to minus 225, equal to a reduction of 246 ; and a compound was produced having the formula $4\text{K}_2\text{CrO}_4, \text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl} \text{ KCl}$. In this case, as in all others where the negative electric energy is decreased by the addition of the substance to the water, the combining proportion is indicated by a maximum minus number, instead of by a minimum plus one.

In the next case the energy is still further reduced:—

TABLE VIII.

 $4\text{K}_2\text{CrO}_4, \text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl}, \text{KCl} + \text{K}_2\text{CO}_3.$

	Between 1 part in	Parts of Water	Temp.	1 part in
$4\text{K}_2\text{CrO}_4, \text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl}, \text{KCl} \dots\dots\dots$	-212	and -238	at 14°C	-225
„ „ „ „ „ + $6\text{K}_2\text{CO}_3$	-6,828	-7,651	16	-7,194
„ „ „ „ „ + 8 „ ×	-10,762	-12,109	16	-11,435
„ „ „ „ „ + 10 „ -	7,673	-8,520	16	-8,096
„ „ „ „ „ + 12 „ -	2,476	-2,753	15	-2,614
$\text{K}_2\text{CO}_3 \dots\dots\dots$	-86,111	-96,875	19	-91,493

The electro-negative energy was reduced or electro-positive energy increased from -225 to -11,435, and the compound formed is represented by the formula $8\text{K}_2\text{CO}_3, 4\text{K}_2\text{CrO}_4, \text{Na}_2\text{SO}_4, \text{K}_2\text{SO}_4, 2\text{NaCl}, \text{KCl}$. I did not consider it necessary

to carry the process of reduction further. It would have been easy to have commenced with chlorine, and carried the reduction down to the above negative number; and still further (see "Relative Amounts of Voltaic Energy of Electrolytes," Proc. Roy. Soc. January 17th, 1889).

In all these cases every addition to the degree of complexity of the definite compounds formed by the addition of a proper proportion of a substance of different chemical composition, was attended by a large decrease of the voltaic energy, and the relative amount of electro-negative energy varied inversely with the degree of molecular complexity of the substance.

In the next two instances the successive losses of energy, attending the neutralization of a polybasic substance by successive additions to it of single molecular proportions at a time of a monobasic substance to combine with it, are shown.

TABLE IX.



	Between 1 part in	Parts of Water	Temp.	1 part in
NaCl	193,750	and 221,428	at 15° C.	207,589
$\text{Na}_2\text{HPO}_4 + \text{NaCl}$..	738	820	13	779
2 „ + 3 „ ..	662	738	13	700
1 „ + 2 „ ..	656	724	13	690
2 „ + 5 „ ..	637	704	13	670
1 „ + 3 „ × ..	574	637	13	605
2 „ + 7 „ ..	659	738	13	698
„	1,900	2,214	15	2,057

TABLE X.



	Between 1 part in	Parts of Water	Temp.	1 part in
Na_2HPO_4	1,900	and 2,214	at 15° C.	2,057
$\text{LCl} + \text{Na}_2\text{HPO}_4$	—993	—1,107	13	—1,050
2 „ + „	—1,371	—1,409	13	—1,390
3 „ + „ × ..	—1,476	—1,640	13	—1,558
4 „ + „	—1,230	—1,360	13	—1,295
„	193	215	16	204

In the first of these two cases the successive losses of energy are shown by a series of plus numbers diminishing in value down to the combining proportion ; in the second one the losses are shown by a series of minus numbers increasing in value towards the combining proportion. In each case the relative amount of energy decreases with each successive proportion of substance added until the combining proportion is attained, and then commences to increase.

XLIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 282.]

February 6, 1889.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communication was read :—

1. "On the Occurrence of Palæolithic Flint Implements in the neighbourhood of Ightham, Kent, their Distribution and probable Age." By Joseph Prestwich, D.C.L., F.R.S., F.G.S.

The author stated that Mr. Harrison of Ightham has discovered over 400 palæolithic implements lying on the surface at various heights and over a wide area around Ightham. A description of the physiography of the district and of the distribution of the various gravels and drifts was given, and in the absence of fossils, attention was called to the different levels at which the deposits occurred, and to their physical features and characters. Besides the river-gravels, two groups of unclassified gravels were described, one occupying a low level, and the other levels higher than that to which the river-drifts reach ; the latter is of varied composition.

In the case of the Shode valley, only beds below the contour-level of 350 feet in its upper part, and of 300 feet or less in its lower part can be referred to the former action of the Shode, and those above this belong to a high-level drift of uncertain age. The composition of the various gravels was described in detail.

The implements are found on the surface of the land at all levels up to 600 feet, and Mr. Harrison has discovered them at 40 localities in the hydrographical basins of the Shode, the Darent, the Leybourne stream, and in part of the Thames basin. Two groups of implements extend far beyond the limits assigned to the river-drifts formed since the present hydrographical basins were established, and must be accounted for by some other means than those in connexion with the former *régime* of the existing streams. A description of the general characters and variations observable in the implements was given.

It is evident from the condition of most of the implements that they have been imbedded in some matrix which has produced an external change of structure and colour. In the case of the river-gravel sites the question presents no difficulty. Three classes of implements have been found; (i.) where the flint still shows some of its original colour; (ii.) those of which the surface has turned from black to white, has been altered in structure, and acquired a bright patina, and which show no trace of wear; (iii.) those of which the flint has also lost its original colour, but has been stained, and is with or without patina; these are generally much rolled. The characters of the first call for no comment. Those of (ii.) and (iii.) are very marked, and there is no difficulty in referring each to a distinct matrix. The implements of class ii. have been imbedded in a stiff brick-earth, generally of a reddish colour, and those of class iii. seem to have lain in ferruginous beds of sand or gravel. Reasons were given for supposing the surface to have been once covered with a deposit of clay or loess, since denuded except where preserved in pipes, and that a continuous plane descended from the high range of the Lower Greensand to the Thames valley, which has since been lowered 300 feet or more. It was also shown that the high-level deposits were formed anteriorly to the post-glacial drifts of the Medway- and Thames-Valleys. It is probable that the loess is a deposit from flood-waters, and that some of it may be referred to the Medway flowing at a higher level; but the highest deposits cannot be so accounted for, and the author referred to the possibility of glacial action, without insisting on it. The deposit on the Chalk-plateau is abruptly cut off by the river-valleys, and the rudest forms of implements, such as those of Ash and Bower Lane, occur on this plateau at from 500 to 550 feet, and the author thinks they may possibly be of Pre-glacial age. The changes which have taken place in the physiography of the district, and the great height of the old chalk-plateau, with its clay-with-flints and southern drifts point to long intervals of time, and to the great antiquity of the rude implements found in association with these drifts. That the removal of the material indicates the existence of agents of greater force than those operating under the present river *régime* closes up the time required for the completion of the great physical phenomena, though the author's inquiry tends to carry man further back geologically than is usually admitted.

February 20.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

The following communications were read :—

1. "On the Cotteswold, Midford, and Yeovil Sands, and the Division between Lias and Oolite." By S. S. Buckman, Esq., F.G.S.

After giving a short sketch of the work and opinions of other

writers, the author proceeded with the evidence on which his own views are based. He described a series of sections of the typical exposures of "Sands" and contiguous strata, commencing near Stroud and terminating on the Dorset coast. Dividing the series, into seven horizons, characterized by their distinctive Ammonites, viz.—*Amm. communis*, *variabilis*, *striatulus*, *dispansus*, the genus *Dumortieria*, *Amm. Moorei*, and *opalinus*, and taking the *Striatulus*-beds as a fixed starting-point, the author demonstrated how the strata varied in regard to that horizon. The Cotteswold Sands, containing the *Variabilis*- and part of the *Communis*-horizons, were below the *Striatulus*-beds; the Midford Sands, containing the *Dispansus*-horizon, were above, *Gramm. striatulum* occupying a thin bed at the base; the Yeovil Sands, containing the *Moorei*- and *Dumortieria*-horizons, overlay a bed containing Ammonites of the *Dispansus*-horizon, and were consequently still later deposits.

Since the different sands were deposited not on a horizontal plane, in point of time, but, as it were, obliquely, the deposit of Cotteswold Sands having ceased before that of Yeovil Sands commenced, it was incorrect to lump all the "Sands" from the Cotteswolds to the Dorset coast under the single local name "Midford Sands," thereby implying a contemporaneity which did not exist, while the use of the present restricted local names was defended.

The Ammonites were apparently uninfluenced by changes in the character of the deposit, since the same species are found in Limestone in the Cotteswolds, in Sands at Midford, and in argillaceous Marl at Ilminster. The change from argillaceous to arenaceous or calcareous deposits has been looked upon as so distinct a feature, that it has been utilized as a great argument in favour of drawing the line between Lias and Oolite at that point; but if this be done, the line is always drawn at different horizons in different districts.

If lithology furnishes no reason for a dividing-line at this point, it was shown that neither did palæontology. It was also shown that the Ammonite family Hildoceratidæ dominated the period from the *Falcifer*- to the *Concavus*-zones, and that with the close of the latter zone they died out with singular abruptness, and that, furthermore, there exists, both in England and upon the continent, a marked hiatus at the same point due to the absence of a zone or a number of zones.

On account of these facts the proposal was put forward that d'Orbigny's term "Toarcien" should be employed to designate the strata from the *Falcifer*-zone to the *Concavus*-zone inclusive, that this term should not be used in the sense of merely an extended "Upper Lias," but to mark an entirely distinct transition-formation,—a definite part of the Jurassic period,—separating the typical Lias from the mass of thoroughly Oolitic strata.

2. "On some Nodular Felstones of the Lleyn Peninsula." By Miss Catherine A. Raisin, B.Sc.

This paper dealt with two small masses of rock forming the headlands of Pen-y-chain and Careg-y-defaid, a few miles from Pwllheli. They consist of old lava-flows, once glassy, now devitrified and, at the former place, associated with interbedded agglomeratic and ashy strata. The lithological characters, as well as other slight evidence obtained, would fully support the identification by the Survey of the surrounding beds as of Bala age.

The rocks exhibit evidence of alteration and of silicification, suggesting that the district may have passed through a Solfatara stage, and that its condition may have been not far removed from that of a geyser region. Perlitic structure is common, and is often found in connexion with spherulitic growth, of which there are many gradations, and specially marked and large examples are presented in the agate nodules.

Some nodules seem to result from spheroidal fracture, others to be masses of flow-brecciation; but the majority have a spherulitic crust, often surrounding an interior occupied by secondary quartz or chalcedony. Similar specimens were described and compared which had been received from Boulay Bay, through the kindness of Professor Bonney. The evidence of these and of the Lleyn examples appears to be strongly in favour of the view that the spherulite is the least altered and most durable part of the mass. Other considerations were brought forward by the author, which would offer some further difficulties in accepting the decomposition-theory to account for the origin of the interior of the nodules. Some of the specimens described present certain special characteristics, and, at one locality in the Lleyn, what seem to be quartzose amygdaloids occur, in close relation to agate nodules. On the whole, although the mode of origin is difficult or impossible definitely to prove, the evidence appears to suggest that in these nodules a spherulitic crust has formed around an originally vesicular nucleus.

3. "On the Action of Pure Water, and of Water saturated with Carbonic-acid Gas, on the Minerals of the Mica family." By Alexander Johnstone, Esq., F.G.S.

Two muscovite fragments were suspended for a year, one in distilled water, the other in water saturated with carbonic anhydride. A good deal of mica-dust was detached from each, but no material had been dissolved, the only chemical change being hydration, accompanied by physical alteration, producing a mineral chemically and physically similar to a natural hydromuscovite.

When biotite was similarly treated, the mineral suspended in the distilled water became a hydrobiotite, whilst that in the water

saturated with carbonic anhydride underwent chemical change, and was converted into hydromuscovite by loss of magnesia and iron, which were dissolved in the water.

Lepidomelane in pure water became hydrated, but in carbonated water also sustained a loss of iron.

The author has ascertained that when anhydrous micas become hydrated, or lower hydrated ones more highly hydrated, they increase in bulk.

XLIV. *Intelligence and Miscellaneous Articles.*

ON THE OPHIOLITE OF THURMAN, WARREN CO., N.Y., WITH
REMARKS ON THE EOZOON CANADENSE. BY GEORGE P.
MERRILL.

THE Warren County Ophiolite, or Verdantique Marble, as seen in the limited amount put upon the market, consists in its typical development of an even granular admixture of white calcite and pale yellowish-green serpentine in about equal proportions. The uniformity of texture is, however, often interrupted by large irregular blotches of deep lustrous-green serpentine, which, as shown in a large block in the National Museum collection, sometimes carry a white nucleus. The presence of this nucleal material, which may frequently be observed passing by imperceptible gradations into the green serpentinous material, suggested at once that here, too, the serpentine is a metasomatic product, as the writer has shown* is the case with that of Montville, New Jersey. Thin sections of the rock under the microscope confirm this suggestion. The white nuclear mineral is seen to be an aggregate of small monoclinic pyroxenes, quite colourless in the thin sections, without pleochroism, but polarizing brilliantly and giving extinctions on clinopinacoidal sections as high as 41° . Irregular canals of serpentinous matter cut through these aggregates following cleavage- and fracture-lines, and all stages of alteration, can, as in the Montville stone, often be observed in a single section. In the more even-textured portions of the rocks the serpentine appears as rounded or oval granules, with small enclosures of secondary calcite imbedded in the large original plates of the same material. Here, too, all stages of alteration are readily detected, some of the pyroxene granules being traversed by but a few wavy threads of the serpentinous matter, while in others not a trace of the original mineral remains. Were it not for these fresh remaining portions one would hesitate to pronounce them pyroxenic derivatives, since they in no case show crystal outlines, but are mere oval

* Proc. U.S. Nat. Mus. vol. xi. (1888) p. 105.

blebs or granules imbedded like shot in the white calcite in a manner quite similar to that of the chondrodite grains in the white limestone from Amity, Orange County, in the same State. The granules are not in all cases isolated, but sometimes occur in groups, or connected by canals of serpentinous matter in a manner strikingly suggestive of the detached sections and groups of *Eozoon* chamberlets, as figured by Dr. Dawson on pages 24 and 28 of his late paper*. Indeed I can but feel, since reading his *résumé*, that, even at this late day, this serpentinization of pyroxene is destined to throw some light on the *Eozoon* problem. This idea is supported by the fact that the fragmental *Eozoon* has been reported from these same formations at Warren County, further by Dr. Dawson's statement that eozoonal masses often occur as "rounded or dome-shaped masses that seem to have grown on ridges or protuberances, now usually represented by nuclei of pyroxene"†. While, from the study of so limited an amount of the Warren County stone, it may not be advisable to assert that the remarkably regular structures figured by Dr. Dawson are due wholly to alteration *in situ* of pyroxene granules, I can but suggest that we have in this alteration the source of the serpentinous material, and that the "mineral pyroxene of the white or colourless variety occurring often in the lower layers and filling some of the canals" of the *Eozoon* is but the residual mineral which has escaped alteration. Further, that the structureless nodules of serpentine found in the eozoonal rocks, and to which often patches of *Eozoon* are attached or imbedded, are but patches in which the alteration is complete and the pyroxenic nucleus quite obliterated. Dr. Dawson, although recognizing the frequent accompaniment of a white pyroxene with the eozoonal structure, in no case mentions appearances indicating that the serpentine is an alteration-product, but seems rather to regard it as an original injection‡, following in this respect the well-known teachings of Dr. Hunt§. Those conversant with the literature of the subject may recall that Messrs. King and Rowney|| recognized also the presence of pyroxenes in these limestones, and, in insisting upon the inorganic nature of the *Eozoon*, compared its structural forms to those assumed by chondrodite, coccolite, &c. in the limestones of New York, New Jersey, and other localities. These authorities seem, however, to have regarded the serpentine as true "replacement pseudomorphs" after these minerals rather than alteration or metasomatic products.

* "Specimens of *Eozoon Canadense* and their Geological and other relations." Peter Redpath Museum. Notes on Specimens, Sept. 1888.

† *Op. cit.* p. 29.

‡ *Op. cit.* pp. 16-22 et als.

§ Quart. Journ. Geol. Soc. of London, vol. xxi. p. 67; Chem. and Geol. Essays &c.

|| Quart. Journ. Geol. Soc. of London, vol. xxv. p. 115; also Proc. Royal Irish Academy, vol. x. part iv. (1870) p. 506.

In conclusion, the serpentine in the Warren County Ophiolite, Ophicalcite, or Verdantique, as it has been variously called, is an alteration or metasomatic product after a mineral of the pyroxene group. The original rock would appear to have been simply a pyroxenic limestone, the pyroxene occurring either in scattering granules, or in granular aggregates of considerable size. An examination of the Essex County Ophiolite reveals a somewhat similar though more complicated condition of affairs. A portion of the serpentine here is also derived from a pyroxene; but another, and in cases a very large portion, is apparently after a mineral which I have not as yet found sufficiently unchanged to be able to identify. The rock is as yet insufficiently studied, and must be made the subject of another paper. I am indebted to Mr. George F. Kunz for the Warren County material.—*American Journal of Science*, March 1889.

ON ELECTRODES WITH DROPPING MERCURY.

BY PROF. W. OSTWALD.

In the Philosophical Magazine, 1886, vol. xxii. p. 70, I published a preliminary notice on the use of electrodes with dropping mercury, for measuring directly the potential of electrolytes. Some time afterwards Dr. James Moser employed the same method, but, in my opinion, he did not succeed in properly preparing the surface of his electrodes; an operation by no means easy. Dr. Moser* obtained numbers differing from mine, and ascribes the fault to my having erroneously interpreted a formula of Lippmann.

If, in this formula,

$$x = e + x_0,$$

x is the difference of potential with the small surface of mercury, x_0 the same with the large surface of the capillary electrometer, and e the compensating force. Dr. Moser takes exception to my having neglected x_0 in my determinations.

The experiment which I have made consists in varying the external force e until the surface-tension of the small surface of mercury attains its maximum value. In this case, according to a theorem of von Helmholtz, the value of x becomes null, and we have

$$e = -x_0.$$

The compensating force is then, the sign excepted, equal to the difference of potential between the mercury and the sulphuric acid in the large surface; that is to say, equal to the difference which

* *Comptes Rendus*, cviii. p. 231 (1889).

in ordinary circumstances is set up between mercury and sulphuric acid.

M. Lippmann found this value equal to about 0.9 volt; I myself have found it to be 0.86 volt. This difference is due to difference of concentration of the sulphuric acid; that which M. Lippmann used was about $\frac{1}{6}$, while that which I used was about $\frac{1}{20}$.

The difference of potential between mercury and sulphuric acid is thus really 0.86 to 0.9 volt, according to the concentration of the acid; if then Dr. Moser found a different value of 0.4 volt, it is a decisive proof that Dr. Moser's electrodes do not furnish the true potential of the electrolytes.—*Comptes Rendus*, February 25, 1889.

ON ELECTROLYSIS. BY MM. VIOLE AND CHASSAGNY.

The decomposition of water by means of a powerful current is accompanied by luminous and calorific phenomena, which were noticed more than forty years ago by MM. Fizeau and Foucault*, and since then investigated by a large number of physicists. The use of a Gramme machine which can furnish 40 amperes with an electromotive force of 110 volts has enabled us readily to obtain these phenomena, to observe them in well-defined conditions, and to call attention to some new facts.

In water containing $\frac{1}{10}$ sulphuric acid (with a smaller proportion the phenomena are less well marked) a platinum wire 4.5 millim. in diameter was immersed. The negative electrode was a platinum wire 1.6 millim. in diameter, which was very slowly immersed in the cold liquid.

If the difference of potential at the binding-screws of the voltmeter is above 32 volts, a luminous sheath is observed about the negative wire, which stands out from the liquid, and in which the disengagement of hydrogen alone takes place. It offers a powerful resistance to the passage of the current, which slowly diminishes as the sheath extends, on penetrating the liquid with the wire, and which suddenly falls the moment the sheath disappears to give place to the ordinary disengagement by bubbles. The following table indicates the maximum length l which the sheath can attain for a given electromotive force E in a cold liquid; I is the indication of a Deprez-Carpentier's ammeter in the circuit.

* *Annales de Chimie*, 3rd series, vol. xi. p. 333 (1844).

l.	E.	I.	EI.	
			Obs.	Cal.
centim.	volt.	amp.		
1	35	0.9	32	32
2	39.3	1.6	63	64
3	41.5	2.4	99	96
4	42.8	3.1	132	128
5	44	3.7	163	160
6	44.8	4.3	192	192
7	45.5	4.8	218	224
8	46			

The quantities of electrical energy expended in the voltameter are virtually proportional to the maximal lengths of the sheath which they can produce.

The light which is seen on the electrode is discontinuous; there are at first only some brilliant orange points at the end of the wire; then a fringe of a violet colour extends over the whole of the immersed part.

This part becomes greatly heated and the heating contributes powerfully to keeping up the sheath. If in fact the current is broken the sheath does not at once disappear, and a hissing is produced when the liquid touches the metal. In like manner the transition from the sheath to bubbles, when the electrode being gradually immersed reaches a certain depth, is accompanied by a kind of explosion. On the other hand, with a potential lower than 32 volts, the sheath can be produced on the negative wire which has been previously heated; but the phenomenon is then transitory, and the disengagement in the form of bubbles soon takes place across the liquid which has come in contact with the cooled wire.

The sheath having been formed for a certain length, if the difference of potential between the two electrodes be made to increase progressively, the calorific and luminous manifestations increase in intensity. The discharges which illumine the sheath become more numerous and more brisk. The disengagement of gas is accelerated; the liquid rises round the electrode; and the sheath increases and becomes detached with evolution of light and explosion in the form of large, compressed bubbles. The immersed part of the electrode becomes red hot and may even melt; with an electromotive force of 80 to 100 volts platinum wire, 1.6 millim. in diameter, immersed to a depth of from 2 centim. to 4 centim., is easily melted, while the part of the same wire which is outside is scarcely hot.

With a great difference of potential it is difficult to produce,

and still more difficult to maintain, the sheath, which at the smallest agitation is transformed into crepitating bubbles.

This resonant form of electrolysis is accompanied by a superficial disaggregation of the negative electrode; the liquid soon becomes charged with a very heavy black powder, which is essentially composed of a hydride of platinum; this decomposes in a vacuum above 400° * and corresponds to the formula Pt_2H .

We have hitherto only been concerned with the phenomena met with at the negative pole. A sheath may also be observed on the positive pole. For this purpose it is sufficient to invert the conditions of the experiment, to attach to the negative pole the wire of 4.5 millim., always deeply immersed in the same water acidulated to $\frac{1}{16}$, and to gradually immerse the positive electrode, which is a wire of 1.6 millim. diameter. There is considerable difficulty in obtaining the sheath, which does not form sharply with less than 50 volts, but which in return is remarkably persistent; it is moreover much less luminous.

If the sheath is first of all formed on the stout wire, which is immersed to a depth of 2 to 3 centim., in proportion as the stout wire is immersed the sheath elongates to the point at which it disappears suddenly; but at the same time the strength of the current increases, and the other electrode may become so much heated as in turn to be covered with a luminous sheath, or with crepitating bubbles.

Similar phenomena, though far less intense, are met with in water acidulated with phosphoric acid. The normal decomposition is disturbed with greater difficulty, as has already been pointed out by M. Mascart †.—*Comptes Rendus*, Feb. 11, 1889.

ON THE ELECTROLYTIC BEHAVIOUR OF MICA AT HIGH TEMPERATURES. BY W. H. SCHULTZE.

The results of this investigation are as follows:—

Mica split parallel to the planes of cleavage shares with glass the property of becoming a better conductor for the current as the temperature rises. After reaching a maximum, its conductivity diminishes; and at a certain high temperature it becomes infinitely small.

Comparing mica and glass, it is seen that even at a high temperature the former is the better insulator.—Wiedemann's *Annalen*, vol. xxxvi. p. 635, 1889.

* Berthelot, *Annales de Chimie et de Physique*, 5th series, vol. xxx. p. 530 (1883).

† *Journal de Physique*, series 2, vol. i. p. 111 (1882).

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[FIFTH SERIES.]

MAY 1889.

XLV. *The Concave Grating in Theory and Practice.* By JOSEPH SWEETMAN AMES, Assistant in Physics in the Johns Hopkins University*.

[Plate VIII.]

SINCE the invention of the concave grating in 1881, it has been universally recognized as one of the most valuable instruments for spectroscopic work at our command. Owing to difficulties in their ruling, however, it is only recently that they have come at all into common use. Thinking that the experience of many years might be useful, at Professor Rowland's request I write this explicit description of the apparatus at the Johns Hopkins University and the adjustments found necessary for its accurate use.

General Theory.

The general theory of a concave spherical grating gives (see Rowland, *Phil. Mag.* vol. xvi. p. 197, and *Amer. Journ. Sci.* vol. xxvi. p. 91) as the radius vector of the focal curve (see Pl. VIII. fig. I.),

$$r = \frac{R\rho \cos^2 \mu}{R(\cos \mu + \cos \nu) - \rho \cos^2 \nu},$$

it being referred to the centre of the grating as origin. μ is the angle r makes with ρ the radius of curvature of the grating; and R and ν are the coordinates of the source of light. For any given value of R and ν there is thus a curve defined

* Communicated by the Author.

Phil. Mag. S. 5. Vol. 27. No. 168. May 1889. 2 B

by r and μ , on which the various spectra are brought to a focus ; and there is a second curve passing through R, ν such that, if the source of light be placed at any point of it, the spectra will be brought to focus along the curve r, μ . These two curves are then conjugate, and their properties have been discussed by Mr. Baily in the *Philosophical Magazine* for 1883 (vol. xv. p. 183).

If we make $R = \rho \cos \nu$ (*i. e.* place the slit on the circle whose diameter is the radius of curvature of the grating), $r = \rho \cos \mu$; that is, the two focal curves coincide. This case is shown in fig. II.

As is well known, this arrangement is mechanically secured by placing the slit at the intersection of two beams set at right angles, on which are ways to carry the grating and eyepiece, these two being kept at a constant distance ρ apart by an iron girder. Thus, in fig. III. the slit is at A, the grating at B, and the eyepiece or camera at C.

The reasons for putting the eyepiece at C, where $\mu = 0$, are easily found. Suppose the micrometer-eyepiece were placed at D (fig. IV.), tangent to the focal circle. Let the eyepiece be displaced along the tangent by an amount $\overline{DD'}$ or " a ,"

$$a = \frac{\rho}{2} \sin 2(\mu - \theta),$$

$$\begin{aligned} da &= \text{one turn of micrometer,} \\ &= \rho \cos 2(\mu - \theta) d\mu = A. \triangle \end{aligned}$$

But by theory of diffraction (see Rayleigh, *Encyc. Brit.*, Wave Theory of Light, vol. xxiv. p. 437),

$$\lambda = \frac{\omega}{N} (\sin \nu + \sin \mu),$$

where ω is grating-space and N the order of spectrum ;

$$\therefore d\lambda = \frac{\omega}{N} \cos \mu d\mu = \frac{\Delta \omega}{N \rho} \frac{\cos \mu}{\cos 2(\mu - \theta)}.$$

Or, if a photographic plate, bent to radius $\rho/2$, were placed at D, one scale-division Δ along plate

$$\begin{aligned} &= \rho d\mu, \\ d\lambda &= \frac{\omega}{N} \cos \mu d\mu, \\ &= \frac{\Delta \omega}{\rho N} \cos \mu. \end{aligned}$$

Now, if $\theta = 0$ (*i. e.* if the micrometer-eyepiece or the camera-

box be placed perpendicular to the arm \overline{BC}), we have

$$d\lambda = \frac{\Delta\omega}{\rho N},$$

since μ is so small that we can put $\cos \mu = 1$. Hence the spectrum is "normal" at C. Further, in this case,

$$\lambda = \frac{\omega}{N} \sin \nu;$$

$$\therefore \text{since } AC = \rho \sin \nu,$$

$$\lambda = \frac{\omega}{\rho N} \overline{AC}.$$

Thus, if one absolute wave-length is marked on \overline{AC} , and the instrument is in perfect adjustment, we can mark on the beam \overline{AC} a scale of wave-lengths for each spectrum, and the absolute wave-length of any line is known at once. It is important to notice that this scale on the beam is identical with the scale on the photographic plate, and that all the spectra are in focus at C at the same time, and *stay in focus* however C moves along \overline{AC} , it being rigidly fastened to B. These facts alone would render a concave grating preferable to a plane one; but it has many other points of superiority. It is the only spectroscope suitable for use in both the ultra-violet and the infra-red. Much longer photographic plates can be used than with any other instrument, since they can easily be bent so that they are entirely in focus. Between the slit and the camera-box no lens is interposed. Besides the saving in light and cost, there are no corrections necessary for spherical aberration, imperfections of lenses, right- and left-handed quartz, &c. Further, the concave grating is *astigmatic*, *i. e.* a point of light as the source is brought to focus, not in a point, but in a line. The advantages of this fact are:—

(1) A narrow spark at the slit is broadened out into a wide spectrum.

(2) Greater accuracy in comparing metallic and solar lines, as will appear later when the use of the instrument is described.

(3) No "dust-lines," as they are brought to a different focus.

(4) A spectrum is obtained which is broad enough to stand enlarging.

Theory of Errors in Adjustment.

The mounting of the slit, grating, and camera-box on the circumference of a circle of radius $\frac{\rho}{2}$ passing through the

centre of the grating is the ideal one. In practice it is impossible to attain it; and so it becomes necessary to study the effect of any small displacement from the perfect adjustment.

I. Suppose ρ slightly less than the fixed arm \overline{BC} (fig. V.).

$$BC = a,$$

$$BD = \rho.$$

We wish to find r in the neighbourhood of $\mu = 0$;

$$\therefore r = \frac{\rho R}{R + R \cos \nu - \rho \cos^2 \nu}.$$

But we keep $R = a \cos \nu$;

$$\therefore r = \frac{\rho a}{a + a \cos \nu - \rho \cos \nu}.$$

Let $a = \rho(1 + \theta), \text{ i. e. } \theta = \frac{CD}{DB},$

$$r = \rho \frac{1 + \theta}{1 + \theta(1 + \cos \nu)}.$$

If θ is small,

$$r = \rho(1 - \theta \cos \nu).$$

Let the camera-box be placed in focus when $\nu = 0$; its distance from the grating is then $\rho(1 - \theta)$, \therefore the distance it is out of focus for any position ν is

$$y = \rho(1 - \theta \cos \nu) - \rho(1 - \theta) = \rho\theta(1 - \cos \nu) = a\theta(1 - \cos \nu).$$

Put $\overline{AC} = x = a \sin \nu$;

$$\therefore y = a\theta - \theta\sqrt{a^2 - x^2}.$$

This is the equation of an ellipse having centre at $(0, a\theta)$, and having as semiaxes a and $a\theta$ (fig. VI.).

II. Suppose the slit slightly displaced from A along \overline{AB} (fig. VII.);

$$BD = R,$$

$$AD = b.$$

As before,

$$r = \frac{\rho R}{R + R \cos \nu - \rho \cos^2 \nu}.$$

But $\rho \cos \nu = R + b$;

$$\therefore r = \frac{\rho^2 R}{\rho R - Rb - b^2}.$$

Let $\frac{b}{\rho} = \alpha$, a small quantity;

$$\therefore r = \rho(1 + \alpha)$$

for all values of R removed from 0, as it always is in practice.

Hence, if the camera-box is once put in focus, it stays so, wherever it is moved along \overline{AC} .

III. Suppose the two beams \overline{AB} and \overline{AC} make an angle $\pi/2 - \theta$ with each other (fig. VIII.).

As before,

$$r = \frac{\rho R}{R + R \cos \nu - \rho \cos^2 \nu}.$$

And

$$\rho \cos (\nu - \theta) = R \cos \theta ;$$

$$\therefore \rho \cos \nu = R - \rho \sin \nu \tan \theta ;$$

$$\therefore r = \rho \frac{\rho R}{\rho R + R \rho \sin \nu \tan \theta},$$

if θ is small,

$$= \rho (1 - \sin \nu \tan \theta).$$

If camera-box is put in focus when $\nu = 0$, it will be out of focus at any point by an amount,

$$y = \rho (1 - \sin \nu \tan \theta) - \rho = -\rho \sin \nu \tan \theta.$$

But

$$x = \overline{AC} = \rho \frac{\sin \nu}{\cos \theta} ;$$

$$\therefore y = -x \sin \theta = -x \tan \theta,$$

the equation of a right line making an angle θ with axis of x (fig. IX.).

IV. Suppose the grating turned on its axis so that its radius of curvature makes a constant angle α with the arm BC (fig. X.).

$$BD = \rho,$$

$$BC = a.$$

Since μ is kept equal to α ,

$$r = \frac{R \rho \cos^2 \alpha}{R (\cos \alpha + \cos \nu) - \rho \cos^2 \nu}.$$

But $a \cos (\alpha + \nu) = R$

$$\therefore r = \rho \frac{a \cos (\alpha + \nu) \cos^2 \alpha}{a \cos (\alpha + \nu) (\cos \alpha + \cos \nu) - \rho \cos^2 \nu}.$$

Put $a = \rho (1 + \delta)$, and suppose both α and δ to be small. Then

$$r = \rho (1 + \alpha \sin \nu - \delta \cos \nu).$$

Let the camera-box be placed in focus when $\nu = 0$; the

distance it is out of focus at any point is then

$$y = \rho(1 + \alpha \sin \nu - \delta \cos \nu) - \rho(1 - \delta)$$

$$= \rho(\alpha \sin \nu + \delta - \delta \cos \nu),$$

$$x = a \sin(\alpha + \nu);$$

$$\therefore y = \alpha x + a\delta - \delta\sqrt{a^2 - x^2}.$$

Since α and δ are both small, this curve is the sum of those found in Cases I. and III.

V. Suppose the slit is displaced along \overline{AC} . See fig. XI.

We have

$$AD = b,$$

$$DC = x.$$

As before,

$$r = \frac{\rho R}{R + R \cos \nu - \rho \cos^2 \nu}.$$

$$\text{But } R^2 = \rho^2 - x^2 - 2bx,$$

$$\text{and } \cos \nu = \frac{\sqrt{\rho^2 - x^2}}{\rho}, \text{ since } \frac{b}{\rho} \text{ is small.}$$

$$\therefore r = \rho \left(1 + \frac{bx}{\rho \sqrt{\rho^2 - x^2}} \right),$$

$$y = r - r_{\nu=0} = \frac{bx}{\sqrt{\rho^2 - x^2}}.$$

By the principle of addition of small displacements, the effect of any combination of these four displacements can be found by addition—one can be used to counteract another, and so on. Thus, displacement IV. can correct a combination of I. and III. This has been found true in practice.

Any small displacement, as long as the distance from the grating to the camera-box is unaltered, does not affect the constant of the instrument (*i. e.* the ratio of Δ to $d\lambda$), for, as we saw above, that depends on this distance alone.

General Description.

Before giving the adjustments and precautions necessary in mounting a concave grating properly, I will briefly describe the various parts of the apparatus as used in Professor Rowland's Laboratory.

The instrument is mounted in a room, the walls and fixtures of which are blackened, and whose windows are of "ruby" glass and provided with black shades. Opening off

this is a balcony for the heliostat. The beams carrying the instrument are placed about eight feet from the floor, and a platform erected at one end of the room, thus allowing the floor-space to be used for other purposes if necessary.

AB and AC (see figs. XII., XIII.) are heavy wooden beams 6×13 in. and 23 feet long. AB is fastened rigidly to the wall, while AC has a slight freedom of rotation about A, controlled by screws at C. The "ways" for the grating-holder and camera-box are fastened to these beams by screws which admit of adjustment, so that the "ways" may be straightened if the beams warp. They are made of $\frac{1}{2}$ -inch angle-iron, although a board made of any hard wood may be used. G G' is a 4 in. tubular wrought-iron girder, braced by a truss, and pivoted at its ends, directly over the "ways," on two iron carriages. Its length is approximately equal to the radius of the grating, and has a range of adjustment of about six inches. The carriages have each two brass wheels or rollers placed nearly a foot and a half apart, and these resting on the iron ways enable the girder to be easily moved from one position to another. The camera-box and grating-holder are themselves movable along BC and have freedom to revolve around axes, but can be finally clamped in place. The camera-box (see fig. XIV.) consists of a fixed wooden frame B, and a box A which can be removed. The sensitive plate is placed in A in suitable slots and is pressed firmly by means of wooden buttons against pieces of hard rubber so that it is bent to the proper radius. There is in B a frame which can be moved vertically by a rack and pinion; and to this A is fastened by dowel pins on the bottom and hooks at the top. On the back of the camera-box, B, is hinged a board "C," which can be held firmly in place by hooks. This board carries a brass plate (see fig. XV.) having a longitudinal opening of a width equal to the thickness of the plate and capable of revolution around a horizontal central axis. By means of stops this revolution is confined to 90° . This plate is used for the comparison of spectra, as described below.

The grating-holder is made of brass. It consists (see fig. XVI.) of a heavy platform carrying an upright frame, B, which can move in slots on A. To B is fastened by screws at the sides, P, a square piece of brass D. D is movable around the axes, P, by means of a screw S. To D by means of an axis P', at the bottom is fastened the frame, C. By means of a screw at S' whose nut is rigidly connected with D, C can be moved around the axis, P'. Springs take up the slack of the screws when unscrewed.

The grating itself stands on two projections at the bottom

of C, and is held there, *free from all constraint*, by a soft wax. By means of the side and back screws the grating can, then, be turned around its centre in its own plane, or tipped back and forward.

The slit placed at A is of somewhat complicated mechanism. See fig. XVII. It has the following adjustments:—

1st. Width of slit can be regulated by a micrometer-screw. It is generally not open more than 0.001 in.

2nd. The slit can be rotated about a central axis so as to make it parallel to the lines of the grating. This adjustment is one of the last to be made in mounting the grating, and is done by turning the slit until the definition is the best possible. This is most important, as the excellence of the photographs depends largely upon it. The definition is spoilt, if the slit is $0^{\circ}5$ out.

3rd. Stops can be inserted at top and bottom, thus causing the grating to be illuminated by the centre of the solar image only. Otherwise the definition may be spoilt by the rotation of the sun. It is important, therefore, that the image of the sun on the slit be quite large. With the larger apparatus in use in the Johns Hopkins University it is 1.2 cm. in diameter, and this is reduced one half by the stops.

For solar work a heliostat, having a south exposure, throws the light on the slit by means of a condensing-lens and a totally reflecting prism. The lens is held in a brass frame, and can be adjusted from within the building. Between the prism and the lens is a revolving stage with circular openings, across which absorbing solutions can be placed. Both this stage and the lever arm carrying the reflecting prism are controlled by strings running along \overline{AC} ; so that, without leaving his seat, the observer can place different solutions before the slit, or put aside the prism, when a metallic spectrum is to be photographed. For this purpose, along the line of the slit and grating is a wooden tube with condensing-lens, which focuses on the slit the image of the arc-light or spark, placed in a separate compartment. (See fig. XII.) All lenses and prisms must, of course, be made of quartz.

For the arc-light a Weston dynamo of 150 volts, 30 amperes power is used, or alternating Siemens of 700 volts maximum. For spark spectra, Professor Rowland has had constructed an induction-coil, which (with from 3 to 12 gallon-jars) gives a spark of intense brilliancy, when driven by the alternating Siemens dynamo. Using this coil, iron wire $\frac{1}{16}$ in. diameter melts, and $\frac{1}{8}$ in. wire is heated red hot.

Gratings with 10,000, 14,438, and 20,000 lines to the inch are used. For ordinary purposes a 10,000 one is sufficient,

while for photographing in the ultra-violet it is best to have a 20,000 grating, with a ruled space of $5\frac{1}{2}$ in. on a 6 in. polished surface. The radius of curvature is generally 21.5 ft. The photographic plates are 19 in. long, 2 in. wide, and $\frac{1}{4}$ in. thick. This thickness allows the plates to be bent to the required radius without breaking. They are flowed with an ammonia emulsion by Professor Rowland himself, and register from 15–20 on Warnerke's sensitometer. Quick plates give too coarse an effect for enlargement. For short focus gratings, such as may be used for gaseous spectra or direct stellar spectra, Professor Rowland has suggested the use of paper or celluloid negatives, as they can be bent to a small radius.

The micrometer-eyepiece used is more like a dividing-engine than an ordinary micrometer. It has a run of 5 inches, and the screw is to all purposes perfect, having been made according to the directions given by Professor Rowland in his article on the Screw in the *Encyc. Brit.* vol. xxi. p. 552.

Hoods of black cloth to keep out stray light are necessary at the slit, and at the camera-box, where one should extend halfway to the grating, as even the darkest room has some light in it.

Adjustments.

The adjustment of these various parts of the apparatus is comparatively simple. The two beams carrying the grating and camera-box are made as level as possible and placed at right angles by the "3, 4, 5" rule. The two axes at the ends of the girder must be made parallel, *while the girder is under stress*. To do this the girder is supported at its ends on two "horses;" and the axes are adjusted by the control-screws until the two are vertical. This is the most difficult adjustment.

The camera-box, grating-holder, and slit are put in place at the proper height. Most gratings give a brighter spectrum on one side than on the other; and so, before placing the grating on its holder, it must be examined to see which side should be used. A candle is held at the centre of the camera-box, which is kept directly over the axis of the carriage; and the grating is turned and the girder lengthened until the flame and its image coincide. By this, the grating is placed perpendicular to the girder, and the girder itself is given the correct length. The camera-box is then made vertical by a plumb-line. To adjust it perpendicular to the girder, a piece of plate-glass is fastened to its face, and a candle is held on the girder near the grating. The camera-box is then revolved

until the flame and image come in line. The reflecting-prism is now put in place so as to illuminate the *entire* grating, and the slit opened. The spectrum formed at the camera-box is observed by the eye, or thrown on a piece of paper; and the back-screw of the grating-holder is turned until it falls at the right height. The camera-box is moved along its way, and in general the spectrum rises or falls; and this is corrected by the side-screw of the grating-holder. These two adjustments are repeated many times until the spectrum stays in place however the camera-box is moved. Then the slit is narrowed, and revolved until the best definition is secured. The instrument now should be in perfect adjustment; and to test this an exposed photographic plate, of which the emulsion has been partly scraped, giving it a lattice-work appearance, is put in the camera-box, emulsion side toward the grating. The spectrum formed on the plate and the emulsion itself ought now to be in focus at the same time in all orders of spectra; that is, if the plate is observed with an eyepiece, there should be no parallax between the two. In general, further adjustment is found necessary. It was to this end that the theory of errors, as above given, was deduced. Let the camera-box be placed in focus when it is near the slit; and then, as it is moved away from it, suppose the parallax increases proportionally to the distance along the way. This would lead one to think that the two beams were not exactly at right angles. Similarly for the other displacements. It is found in practice that it does most good to turn the grating-holder slightly around its vertical axis.

If, in setting up the instrument, a micrometer-eyepiece is used instead of a camera, practically the same adjustments are found necessary.

Use of Instrument.

Gratings in Practice.—Special gratings should be selected for special purposes. Every grating has spectra of different brightness on the two sides; and one should be used which is bright in the particular spectra desired. But more than this, even if the red of any one spectrum is bright, the violet may not be. This fact must be especially noted in working beyond the visible spectrum. Further the various parts of the grating, especially if it is concave, may give spectra of varying brightness. For instance, the second spectrum may be uniformly bright for all parts of the grating, while one end of the grating may give a bright third spectrum and the other end a faint one. This fact may be brought out by viewing the grating directly with the eye. It is only when extreme

accuracy is wished and the overlapping spectra of different orders are to be used, that this imperfection must be guarded against. Since a 10,000 grating has on the whole better definition than a 20,000 one, and as it is much cheaper, it is better to use one in all cases when possible. For use with the micrometer-eyepiece, when of course the ultra-violet spectra do not interfere, one can always be used.

It is only when work is to be done with the camera in the ultra-violet part of the spectrum that it becomes necessary to use a 20,000 grating. This is due to the fact that, for the same dispersion with a 20,000 grating as for a 10,000, there are fewer overlapping spectra. The range of concave gratings mounted as above are as follows :—

Lines per inch.	First Spectrum.	Second Spectrum.	Third Spectrum.	Fourth Spectrum.	Fifth Spectrum.
10,000	Entire.	Entire.	Entire.	To 6,000	To 4,800
14,438	Entire.	Entire.	To 5,760	To 4,330	To 3,460
20,000	Entire.	To 6,000	To 4,000	To 3,000	To 2,400

These limits are taken at the *centre* of the photographic plate. At the end of the plate the limit is somewhat greater, being 6260 in the 2nd spectrum for a 20,000 grating.

With a grating of 21·5 ft. radius, the width of the spectrum varies from $\frac{1}{4}$ in. to 4 in. In the green of the 1st spectrum of a 20,000 grating it is $\frac{3}{4}$ in., and in the green of the second it is $2\frac{5}{8}$ in. This gives an idea as to the width of the photographic plate which is required.

The scale of the negatives in the various spectra, with gratings of 21·5 ft. radius, is as follows :—

Lines per inch.	Scale of Spectra as compared with Ångström's Map.			
	1st.	2nd.	3rd.	4th.
10,000	·26	·51	·77	1·03
14,438	·37	·75	1·12	1·50
20,000	·52	1·03	1·55	2·07

i. e. using a 20,000 grating in the 3rd spectrum, the scale is 1·55. This means that 1·55 millim. on the photographic plate

includes 1 Ångström unit. For gratings of 10 ft. radius, the scale is diminished in the ratio of 100 : 215 or 20 : 43.

Since with a concave grating all the spectra are in focus at the same time, it is important to know what wave-lengths of the different spectra are on the photographic plate or in the field of the eyepiece, for any position they may be in. For this purpose I have given a diagram of the overlapping spectra on the Plate. This explains itself:—Wave-length 6000 in the 2nd spectrum coincides with wave-length 4000 in the 3rd spectrum, with wave-length 3000 in the 4th spectrum, and so on. The vertical lines give the range of the different gratings, as explained above. If it is desirable to cut off any interfering spectrum, glass plates or absorbing solutions may be used. A list of the principal absorbents, and the parts of the spectrum which they *let through*, is given below.

Greenish plate glass	3300–8000
Salicylic acid in alcohol, saturated in quartz cell	3500–8000
Aesculin, 1 gr. in 1 oz. water, with one drop of ammonia—fresh	4100–8000
Potassium ferrocyanide	4400–8000
Primrose or Aniline yellow	5000–8000
Fluoresceine or Chloride of gold	5200–8000
Chrome alum	} 3200–3700 and 4600–5200
Malachite green	
Bitter-almond green	
Brilliant green	
Cobalt chloride	3400–4500
Gentian violet, strong	3600–4600
	and 6000–8000
Potassium permanganate	3900–4600
	and 5800–8000

For example, using a 10,000 grating and photographing in the 4th spectrum, the following absorbing solutions are used at the places specified:—

- At
 3800 Cobalt chloride in water.
 4000 Cobalt chloride or Gentian violet in water in glass cell.
 4200 Potassium permanganate or Gentian violet in water.
 4400 Aesculin or Potassium permanganate.
 4600 Aesculin.
 4800 Aesculin and Malachite green in water.
 5000 Aesculin and Potassium ferrocyanide.
 5200 Aesculin and Potassium ferrocyanide.
 5400 Aesculin and Primrose.

Before using a solution, an observer should always see what its effect is by a preliminary experiment.

Methods of Work.

A spectroscope is used for two purposes—to measure the lines in solar or metallic spectra, or to establish coincidences simply. For both of these, the concave grating is far superior to any other on account of the overlapping spectra.

The micrometer-eyepiece, of course, can be used only in the visible spectrum, while the methods of photography give us this and the invisible too. Rowland's micrometer-eyepiece, as noted above, has a run of 5 inches, and so can include a great number of lines. When a metallic spectrum is to be measured, the solar spectrum is turned on, a series of measurements is taken, then the metallic spectrum replaces the solar, another series is taken, then finally a series of solar lines. All this is done in one run of the screw, and without the observer leaving the eyepiece. The solar lines are found on Rowland's map, and then the wave-lengths of the metallic ones are deduced by interpolation. This same method of interpolation will also give the relative wave-lengths of the solar lines, using the overlapping spectra. The probable error of a wave-length determined this way is ± 0.01 Ångström unit.

Now that we have Rowland's map and his list of solar lines, the photographic process for the measurement of metallic spectra is generally used as far as the erythrosin plates extend or to the D line, although those expert in the use of cyanine plates may photograph below C or even A, as Mr. Burbank has shown in the *Phil. Mag.* for Oct. 1888.

Owing to the astigmatism of the grating, it is not possible to adopt the usual method of illuminating part of the slit with the solar image and part with the spark or arc; and so a different and far better plan is adopted. A compound photograph of the two spectra is taken in the following manner:—The brass plate on the back of the camera-box (see fig. XIV.) is placed vertical, the solar spectrum is photographed along the middle of the sensitive plate, the sunlight is turned off, the brass plate is revolved through 90° , and the metallic spectrum is allowed to fall along the upper and lower parts of the photographic plate. Then, finally, the sunlight is turned on again along the middle of the plate. If there has been any gradual displacement of the camera during the operation the error is eliminated by this process, if the two times of exposure to the solar spectrum are the same.

It is important to notice that record must in all cases be

kept of thermometer- and barometer-readings ; for the corrections due to variations in temperature and pressure may be considerable.

Since no absorbing solution is known which lets through the ultra-violet rays alone, the following method has to be used to determine what lines on any negative are ultra-violet ones. A compound negative, as just described, is taken, having all the overlapping spectra at the point in question along the middle of the plate, and the visible lines alone, obtained by inserting absorbents, along the top and bottom. Those lines present in the first and not in the second are then ultra-violet ones.

The time of exposure varies. For arc or solar light five minutes is the average time required for the most sensitive part, in the third spectrum on plates registering 18 on Warnerke's sensitometer. Ten minutes are required above the D lines in the second spectrum, using erythrosin plates. One hour is needed for cyanine plates, photographing down to the C line. As a practical example, the entire iron and solar spectra were photographed in the second and third spectra from the D lines down to the extreme ultra-violet in nine hours. This includes time spent in developing. Thirty plates, each 19 inches long, were exposed, giving of course many duplicates. Only 10 plates are necessary in the second spectrum of a 20,000 grating for the whole spectrum from the D line to the extreme ultra-violet, wave-length 2000. In one case Liveing and Dewar used 170 plates for the ultra-violet spectrum alone.

With a very powerful induction-coil, worked by a Siemens alternating dynamo, with 6 gallon-Leyden jars, 10 minutes is enough in the most sensitive part and 30 in the extreme ultra-violet, wave-length 2200.

A compound negative taken in the above manner is placed on a dividing-engine, and measurements made on the lines of the two spectra, using a low-power microscope with a single stretched cross-hair. Since the solar spectrum continues down to 3200, the same orders of the two spectra can be compared thus far. Beyond this it is necessary to use different orders. For instance, wave-length 2800 in the third spectrum can be compared with solar lines about wave-length 4200 in the second. This same method is used to determine the relative wave-lengths of the solar spectrum.

To enlarge photographs with a scale of wave-lengths, like Rowland's map of the spectrum, one must proceed as follows.

To make the scale, a thick plate of glass, slightly longer

than the negative, is albumenized and then treated with collodio-chloride. It is then put in any developer until it turns black. A longitudinal strip of the width of the negative is scraped off, and on the edge of this strip the scale is ruled with a dividing-engine. The negative is clamped in place to this scale, and together they are put in the enlarging camera. The accuracy with which the scale can be made and the negative fitted to it is most satisfactory. On Professor Rowland's new map the greatest error is $\cdot 03$ of an Ångström unit, and the probable error is less than $\cdot 02$ of a unit. If the scale is, say, $\cdot 0001$ too large or too small, the photographs can be made to fit the scale by altering the distance between the grating and camera-box by $\cdot 0001$ of its amount, and then focusing by moving the slit in or out.

When this scale is once made, it can be used to give direct readings for the wave-lengths of the lines on any negative simply by placing the negative on the scale.

A word should be said as to the difficulties of ruling gratings, which may explain why so many orders for gratings remain unfulfilled. It takes months to make a perfect screw for the ruling-engine, but a year may easily be spent in search of a suitable diamond-point. The patience and skill required can be imagined. For the past year all attempts to find a point for the new ruling-engine have failed, and it is only within a few days that one has been found. Most points make more than one "furrow" at a time, thus giving a great deal of diffused light. Moreover few diamond-points rule with equal ease and accuracy up hill and down. This defect of unequal ruling is especially noticeable in small gratings, which should not be used for accurate work. Again, a grating never gives symmetrical spectra; and often one or two particular spectra take all the light. This is of course desirable if these bright spectra are the ones which are to be used. Generally it is not so. These individual peculiarities of gratings were fully treated by Professor Rowland in his lectures during the spring term of 1888; and have been embodied by him in a complete mathematical theory of the grating, which he has nearly ready for publication. It is not easy to tell when a good ruling-point is found; for a "scratchy" grating is often a good one; and a bright ruling-point always gives a "scratchy" grating. When all goes well, it takes five days and nights to rule a 6-inch grating having 20,000 lines to the inch. Comparatively no difficulty is found in ruling 14,000 lines to the inch. It is much harder to rule a glass grating than a metallic one; for to all of the

above difficulties is added the one of the diamond-point continually breaking down. For this reason Professor Rowland has ruled only three glass gratings, one of which has been lost, and the other two are kept in his own laboratory. These two were used by Dr. Bell in his determination of the absolute wave-length of the D lines.

Baltimore, March 27.

XLVI. *On Helmholtz's Theory of Mercury-dropping Electrodes, and the Difference of Potential between Clean Mercury and Electrolytes.* By J. BROWN, Belfast*.

WITHIN the last few years considerable attention has been attracted on the continent by a hypothesis put forward by von Helmholtz†, regarding mercury-dropping electrodes as a means of testing potentials of liquids.

The hypothesis is given in § 8 below. Researches based on it have been undertaken by several authors, but chiefly by Ostwald, whose short communication‡, published in 1886, first drew my attention to the subject. His complete work§ gives tables of contact potential-difference for many metals and liquids; and the conviction, on electrochemical grounds, that these were incorrect led to an examination of the hypothesis on which they were based, and to a conclusion respecting the action of mercury-dropping electrodes different from that of von Helmholtz.

The recent publication of a paper|| by Exner and Tuma, in which these authors come to the same conclusion, encourages me to give a *résumé* of the arguments on both sides, beginning with a very much condensed translation of the portion of von Helmholtz's paper referring to the subject (p. 933), of which I shall endeavour to give as correct a rendering as is possible in a few words. The quotation-marks are for convenience, and are not to be taken as implying verbal exactness.

2. "The current which occurs on unequal [successive] immersion of similar electrodes in the same liquid is due to the alteration produced by local action on the surface of the first-dipped electrode. A similar action occurs when mercury

* Communicated by Dr. Oliver Lodge, being a contribution to the Electrolysis Committee of the British Association.

† *Wissenschaftliche Abhandlungen*, i. p. 925 (1882).

‡ *Phil. Mag.* xxii. p. 70 (1886).

§ *Zeitschrift für physik. Chemie*, i. p. 581 (1887).

|| "Studien zur chemischen Theorie des galvanischen Elementes," *Sitzungsberichten d. kais. Akad. d. Wissenschaften in Wien*, xcvii. part ii. p. 1.

is allowed to run out in small drops under the surface of a liquid, the surface of each drop forming out of the interior of the pure metal which has not yet been in contact with either air or liquid. According to all Quincke's* observations, the positive current in this case always goes in the direction of the falling drops of mercury.

3. "That is to say, the mercury which collects on the bottom of the containing vessel, and which concentrates on its surface the films that have been produced by the changes referred to, has greater positive potential than the upper, continually renewed surface. Such a difference of potential implies an electric double layer, whose positive half is inside the lower mercury and its negative on the anion of the fluid in contact.

4. "This double layer forms gradually, since it has been shown by Quincke that slow dropping produces weak differences of potential; while, as the speed of dropping increases, a maximum difference of potential is reached, after which increasing the speed of dropping has no further effect. The maximum will occur as soon as the new portions of the upper mercury form into drops so quickly that they cease to be charged sensibly before they break away, and therefore the upper surface remains in a perfectly unchanged state.

5. "According to Faraday's law, the passage of positive electricity into the metal can only occur by electrolysis, in which is concerned some substance having less attraction for positive electricity than the mercury. This may probably be atmospheric oxygen dissolved in the liquid, to which we may ascribe sufficient affinity for negative electricity to draw it out of the mercury and replace it with positive electricity. The slowness of the charging would then be due to the smallness of the quantity of oxygen present and its slow renewal by diffusion. Further experiment is needed to prove this hypothesis. Quincke shows that boiling the electrolyte does not stop the phenomenon, but a very small quantity of oxygen would be enough.

6. "If the effect were due to any component of the electrolyte which is present in large quantity, the charging of the surfaces in contact would take place in an insensibly short time.

7. "Although this is to be understood merely as a hypothesis, it is sufficient for what follows that, under the received conditions, mercury in contact with liquid charges itself, only

* Pogg. *Ann.* cliii. p. 161 (1874).

slowly, positive to the liquid. The slowness of the charging is well shown in Lippmann's capillary electrometer, where the diffusion is limited by the narrowness of the tube, and the fine thread of mercury only slowly creeps to a position of equilibrium different from that of freshly dropped mercury.

8. "From this I conclude that, when a quickly dropping but otherwise insulated quantity of mercury is in contact by the dropping-point with an electrolyte, the mercury and electrolyte can have no difference of potential. If, for example, the mercury were positive, then would each falling drop form on its surface a double layer, taking positive electricity out of the mercury and so making its potential less and less until it became equal to that of the liquid."

9. Instead of the elaborate theory here described, necessitating hypotheses of an admittedly unproved and rather doubtful character, I think it can be shown that there is a much more simple explanation of the action of dropping electrodes. The arrangement constitutes simply a voltaic cell in which the elements are clean mercury, electrolyte, tarnished mercury.

10. Suppose a drop with a fresh surface is made to protrude a little from the funnel-point immersed in a suitable electrolyte, such as dilute sulphuric acid. Electrolytic action at once takes place; a film of the mercury is oxidized or chemically acted on by the anion of the electrolyte, and there is formed an electric double layer, but with the signs of its components the reverse of those supposed by Helmholtz, viz. *negative* in the metal, *positive* in the liquid, the difference of potentials of its components being large because of the cleanliness of the metal; that is to say, the clean upper mercury is at a lower potential relatively to the electrolyte than the tarnished lower mercury, and therefore on joining these two a current flows from the lower to the upper through the connecting wire, and continues till the upper has become as tarnished as the lower. Let the drop now protrude a little further. A corresponding enlargement of the surface takes place, and a portion of fresh untarnished metal is exposed, causing increased electrolytic action and current, which dies away as the surface of the drop becomes again tarnished.

Suppose now the drop to increase slowly and continuously till it separates and falls off. As it increases the surface is continually somewhat freshened, and so a constant but moderate current is kept up. Moderate because the amount of fresh surface being exposed is small compared with that of the already tarnished surface. But if the drop increase quickly the more rapid formation of fresh surface causes an increase

in the current. The limit to this increase occurs when the current which can flow, taking into account the resistance of the arrangement and the small electromotive force available, combined with any purely local action at the surface of the drop, has not time to effectively tarnish it before it breaks away.

11. It follows that in the experiment described in § 4 the potential of the resting mercury remains constant, while that of the dropping mercury falls with the increasing speed of dropping till the maximum is reached, as described in § 10. I give the following considerations as supporting this view and as showing my difficulties in accepting that of von Helmholtz.

12. Since in § 3 the positive potential of the lower mercury is said to be due to the concentration on its surface of the films produced on the drops, and in § 4 that maximum difference of potential occurs when the drops form and break away before they have time to get charged at all, it follows that with quick dropping, producing maximum difference of potential, the alteration of mercury surface forming the required double layer must, according to von Helmholtz's view, take place during the fall of the drops through the liquid and after they have separated from the upper funnel.

With somewhat slower dropping the alteration would be partly before separation and partly during the fall.

In either case the length of fall is a factor in the result, and making it small or zero should at least decrease the current and electromotive force of the arrangement.

And, further, since the current is in both cases carried by the falling drops and not by the electrolyte through which they fall, any variation in the resistance of the electrolyte should have no effect on the current.

13. I have found, however, by experiment, that decreasing the fall increases the current and has no effect on the electromotive force, while decreasing the resistance of the electrolyte increases the current.

A thistle-funnel drawn out to a fine tube whose orifice was about .17 millim. diameter, and which delivered with a head of 42 centim. of mercury (the whole length of the funnel) about $\frac{3}{4}$ cubic centim. per minute, was fixed with its point just below the surface of a column of spring-water $5\frac{1}{2}$ centim. diameter by 33 centim. high, contained in a tall glass over a little mercury at the bottom.

The difference of potential of this bottom mercury and that in the funnel, measured by a quadrant electrometer (giving 72 divisions for the E.M.F. of a Daniell's cell), was, with the

point of the funnel just under the surface of the water ("up"), or within about 3 millim. of the lower mercury ("down"), as follows:—

Up 26 divisions. Down 23·5. Up 23·5. Down 23·5.
Up 23.

The electromotive force is therefore not sensibly influenced by the length of fall.

The somewhat larger first deflexion is perhaps due to a higher degree of oxidation on the lower mercury, afterwards modified by the fresh drops falling on it. This effect is marked when the lower mercury has only a small surface more easily affected by the mixture of fresh mercury.

14. To experiment on the current from this arrangement, the same apparatus was used with the addition of a glass tube of 7 millim. diameter; open at the ends and fixed upright in the glass jar, its lower end a few millim. above the resting mercury. The point of the funnel was placed in this tube just under the surface of the water, and the dropping and resting mercury connected to the terminals of a reflecting-galvanometer. The deflexion was 57 divisions. Lowering the funnel through about 30 centim. increased this to 400 divisions.

Decreasing the length of fall, therefore, increases instead of decreasing the current, and the increase is due here to the decreased resistance of the electrolyte, for when the narrow tube was removed and the same experiment performed in a wide vessel, the current did not sensibly vary, since the chief resistance was then at the small surface of the drops as they formed at the funnel-point.

It was stated by Quincke* that "the strength of this current decreases with increased resistance of the fluid," though the experiment on which he appears to base the conclusion may be open to another interpretation.

The hypothesis that the current is in any way produced by the carrying down of charges on or in the falling drops is negatived also by several of König's experiments, where the dropping mercury did not fall into the resting mercury in connexion with the electrometer but into another part of the apparatus †.

I find also that when the resting and dropping mercury are connected through a galvanometer, the current remains the same whether the drops fall into the resting mercury or not, provided they are formed in the electrolyte.

* Pogg. *Ann.* cliii. p. 203.

† Wied. *Ann.* xvi. p. 35 (1882).

15. The "law" ascribed to Faraday, in § 5, would not, I think, have been recognized by that philosopher.

The mere combination of free oxygen with the metal would not, according to Faraday's view, be an electrolytic action at all, as might be shown by many quotations from his work, *e. g.* "But in considering this oxidation or other direct action upon the METAL itself as the cause and source of the electric current, it is of the utmost importance to observe that the oxygen or other body must be in a peculiar condition, namely in the state of combination; and not only so but limited still further to such a state of combination and in such proportions as will constitute an *electrolyte*"*.

An anion, according to Faraday†, is the substance which "goes to the anode of the *decomposing body*," and the anode is the "negative extremity of the *decomposing body*;" and he expressly repudiates the application of the terms electro-negative or electropositive to substances according as they are urged by the supposed influence of a direct attraction to the positive or negative pole.

The supposition that individual atoms or molecules can possess any specific electric charges of their own appears to me to be of a very speculative character, but if oxygen have any such charge it must surely be a negative one, since it travels towards the positive pole of a voltameter.

16. Referring now to § 6, the charging of the surfaces does take place, according to the view I advocate, in an insensibly short time, and is due to a component of the electrolyte present in large quantities; but, as shown in § 10, it is not immediately connected with the attainment of a maximum difference of potential at a certain speed of dropping; this effect being due not to a gradual charging but to a gradual tarnishing—a phenomenon experimentally demonstrable, and which is not improbably also the cause of the action in Lippmann's electrometer, referred to in § 7.

In § 7 are given the phenomena on which depend the conclusion expressed in § 8.

I have translated § 8 in full as it contains the hypothesis on which so much work has been carried out, but it is evident that it has only the validity of the previous sections on which it is based. Indeed Ostwald, who gives a long description of his arduous work with these dropping electrodes, admits‡ that the question is not whether they assume exactly the same potential as the liquid, but how much the difference is; a

* Exp. Res. i. p. 273. See also p. 252.

† Exp. Res. i. pp. 197–8.

‡ *Zeitschrift für physikalische Chemie*, i. p. 588.

question which he attempts to answer by a comparison of the indication of the dropping electrode with that of a capillary electrometer, and the adoption of several hypotheses which cannot be said to appeal directly to one's sense of exactness.

17. In the paper referred to above by Exner and Tuma these authors refer to the improbability of any considerable current being produced by the mere carrying down of electric charges on the falling mercury drops; and they adduce experimental evidence to show that such charges are not produced by any simple contact action between the mercury and electrolyte, as supposed by Ostwald, who seems to regard the arrangement as analogous to the water-dropping collector of Sir W. Thomson. The conditions, however, are quite different from those of Sir W. Thomson's collector, where the potential measured is that originally existing at the point where the drops form, and is usually independent of any action between the drops and the medium in which they form.

18. Their paper then puts forward as the true theory of the observed current the explanation given in § 9, viz. that the arrangement is simply a galvanic cell: clean mercury—acid—oxidized mercury; and it is further pointed out that the current carries by electrolytic convection hydrogen to the resting mercury whereby the latter is deoxidized.

It would appear therefore that, leaving local action out of account, the oxidizing of the dropping, and deoxidizing of the resting, mercury go on simultaneously; and as the oxide formed on the drops falls on the resting mercury, I presume we must conclude that the ultimate source of current in this cell is the gravity potential of the upper mercury, which is used up in producing the drops, and changes first into energy of surface tension and then into current.

19. In conclusion, it is important to remark that the view here adopted implies that electrolytes, such as dilute acids, which attack mercury, are positive to clean mercury in contact with them; for if not, and if the electrolyte is negative to the dropping mercury, it must (in order that the observed current may be produced) be still more negative to the resting mercury; that is to say, the difference of electric potential is least where the conversion of chemical potential energy is greatest—a rather improbable assumption, and one not borne out by analogies with other voltaic combinations.

If this be admitted, it follows that the results of all those recently made researches which are based on the assumption that mercury is positive to electrolytes, must be considered doubtful.

Among these may be mentioned Ostwald's *Studien zur Kontaktelektricität*, as referred to above.

J. Moser, *Ueber die Zerlegung der electromotorischen Kraft der Elemente in ihre Potentialdifferenzen**, where von Helmholtz's dropping electrodes are employed in testing the potentials.

Pellat, *Mesure de la différence de potential vraie de deux métaux en contact*†. Much of the above reasoning on the cause of the current from the dropping-electrode arrangement applies also to the experimental work of this paper.

König, *Ueber die Beziehung zwischen der galvanischen Polarization und der Oberflächenspannung des Quecksilbers*‡. Some doubt must arise as to the correctness of the conclusions arrived at in this paper, since they are found, as stated towards the end of it, to agree with those obtained by dropping electrodes.

The hypothesis adopted in the main part of the paper may be briefly referred to.

From a mathematical discussion of the electrical forces supposed, on more or less speculative grounds, to be present in a double layer at contact of mercury and electrolyte, it is concluded that at maximum tension of the surface between mercury and another liquid no difference of potential exists between these substances.

Quincke expressly states (p. 204) that the electromotive force has no relation to the capillary constant at the common surface of mercury and liquid.

Exner and Tuma consider (p. 8) that maximum surface-tension occurs simply when the mercury surface is clean, *i. e.* free from oxide &c. If polarized with either O or H, the surface-tension is diminished. No experimental evidence in support of this view is given, but it seems a not improbable assumption.

It seems to me difficult to suppose that the surface of clean mercury can by any means, so long as it remains clean, be brought to the same potential as that of the surface of dilute acid in contact with it. If we assume, as seems generally admitted, that there is a natural contact electromotive force of an electrolytic kind at the dividing surface, this local "*Potentialsprung*" would seem a necessary consequence, and would, if the potentials of any other portions of the liquid and

* *Wiener Anzeiger*, No. 19, p. 232 (1887).

† *Comptes Rendus*, civ. p. 1099 (1887).

‡ *Wied. Ann.* xvi. p. 1 (1882).

metal were equalized, simply produce a current, but would not itself be annulled.

It would only disappear when the surface of contact was so chemically altered as to preclude or fail to produce any electrolytic action, in which case the new substance formed would imply new conditions of surface-tension, and the hypothesis that the alteration in surface-tension could be referred solely to electrical forces on the double layer would be no longer tenable.

Most of these questions (depending, as they do, on a knowledge of the real differences of potential between metals and electrolytes, which has not yet been obtained) would have been set at rest if one could accept undoubtingly the experimental results contained in the last part of MM. Exner and Tuma's paper, which treats of the measurement of the contact-difference of potential of metals and electrolytes.

The potential of the electrolyte is tested by a mercury-funnel dropping within a paper cylinder which is moistened with the electrolyte in question, and in contact with the metal which is put to earth.

To get a correction for the zero of the instrument, the funnel is afterwards made to drop inside a carbon or platinum cylinder also to earth, which is assumed to give zero-potential at the funnel-point. It does so only if it may be assumed that there is no contact-difference between the platinum or carbon and the air or water-film condensed on it; an assumption which, though probably nearly correct, is as yet unproved, and which therefore detracts from the value of the results obtained.

XLVII. *The Strength of the Induced Current with a Magneto-Telephone Transmitter as Influenced by the Strength of the Magnet.* By CHARLES R. CROSS and ARTHUR S. WILLIAMS*.

IT is a well-known fact in practice, as well as an evident consequence of theoretical considerations, that the effectiveness of a magneto-telephone, when used either as a transmitter or as a receiver, varies with the strength of the magnetism of the core. But the relation of the one to the other has never been studied, so far as we are aware.

Our investigations include a study of the changes in

* Communicated by the Authors. From the Proceedings of the American Academy of Arts and Sciences, vol. xxiv. (n. s. xvi.) p. 113.

strength of the current produced by a magneto-transmitter under varying conditions of magnetization, and of the magnitude of the momentary changes in the magnetic condition of the core of the receiving-telephone when subjected to the action of undulatory or other brief currents, as influenced by the strength of the primitive permanent magnetization of the core. The present paper contains only the results of a series of experiments relating to the first of these; that is, to the effect of varying strength in the magnet of the transmitter, the study of the allied problem of the receiver being still in progress.

The apparatus employed consisted of a cylindrical bar of soft iron, about $4\frac{1}{2}$ inches in length and $\frac{1}{4}$ of an inch in diameter, around one end of which was placed a coil of fine wire similar to that used in ordinary telephonic practice. The resistance of this coil was 100 ohms. It was placed in circuit with a ballistic mirror-galvanometer, from whose deflexion the momentary current produced in the coil by any variation in the strength of the core could be determined. The diaphragm, which was in all cases $2\frac{5}{16}$ inches in diameter, was in its usual place opposite the end of the magnet about which the wire coil was wound, and about $\frac{3}{100}$ of an inch from that end. By means of a rod carrying a cam moved by a weight, a rapid inward push of definite amount was given to the diaphragm, thereby inducing a current in the coil already referred to, and so deflecting the needle of the ballistic galvanometer. The soft iron bar was also surrounded by a second helix, through which was passed a current from a storage battery, serving to magnetize the core. A tangent-galvanometer inserted in this circuit gave the strength of the magnetizing current. A magnetometer placed in the prolongation of the axis of the core, which last occupied an east and west position, made known the relative strengths of the field produced by the core under different conditions of magnetization.

Corresponding observations of the magnetometer-reading, and of the current induced when the diaphragm was moved by the cam, were made throughout a widely varying range of strength of field, and the results were represented graphically by constructing a series of curves in which ordinates represent the relative strength of field, and abscissæ the current due to a given predetermined throw of the diaphragm (about $\frac{1}{100}$ of an inch), as ascertained from the readings of the ballistic galvanometer.

One of these curves is shown at 1, fig. 1, the core in this case being a cylindrical bar of Norway iron $4\frac{1}{2}$ inches long

Fig. 1.

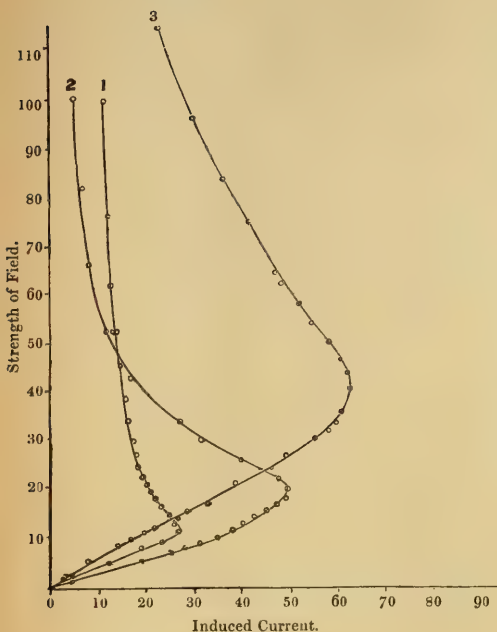
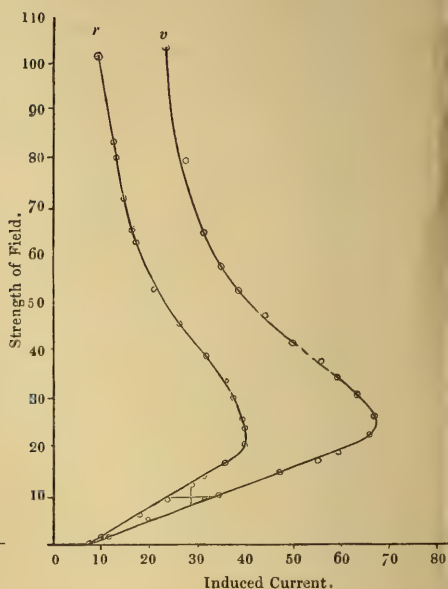
Ferrotypes Iron.

Fig. 3.

Steel.

and $\frac{1}{4}$ of an inch in diameter, and the diaphragm an ordinary disk of ferrotypes iron $2\frac{5}{16}$ inches in diameter and $\frac{1}{10}$ of an inch thick (No. 31 B. W. G.).

Table I. gives the data from which fig. 1 was constructed. The strength of field is given in terms of the tangents of the angles of deflexion of the magnetometer-needle. The induced current is given in arbitrary units, as only relative values are needed. A determination of the value of the deflexions was made by observing the excursion due to the discharge of a condenser through the ballistic galvanometer, and it was found that the abscissa 100 on the curves corresponds to a sudden discharge of approximately 0.00000097 of a coulomb through the coils of the galvanometer.

TABLE I.—Core, Norway Iron.—Diaphragm, Disk of Ferrotypes Iron, No. 31.

Strength of field.	Induced current.	Strength of field.	Induced current.
.....	0·7	·211	20·5
·016	3·3	·229	19·8
·044	12·0	·248	19·2
·058	19·3	·270	18·7
·089	23·3	·302	18·0
·118	27·0	·342	16·9
·141	26·8	·390	16·1
·132	26·6	·454	14·7
·146	25·3	·530	13·3
·164	23·8	·625	12·7
·182	22·5	·773	12·0
·196	21·6	1·014	11·5

Cores of Bessemer steel and of untempered soft steel were also used, with results given in Tables II. and III.

TABLE II.—Core, Bessemer Steel.—Diaphragm, Disk of Ferrotypes Iron, No. 31.

Strength of field.	Induced current.	Strength of field.	Induced current.
·005	2·0	·279	17·8
·030	7·7	·333	16·7
·082	20·7	·396	15·0
·137	26·7	·507	13·5
·160	24·8	·625	11·7
·191	21·2	·748	10·8
·213	19·3	·907	9·7
·248	18·6	1·099	8·7

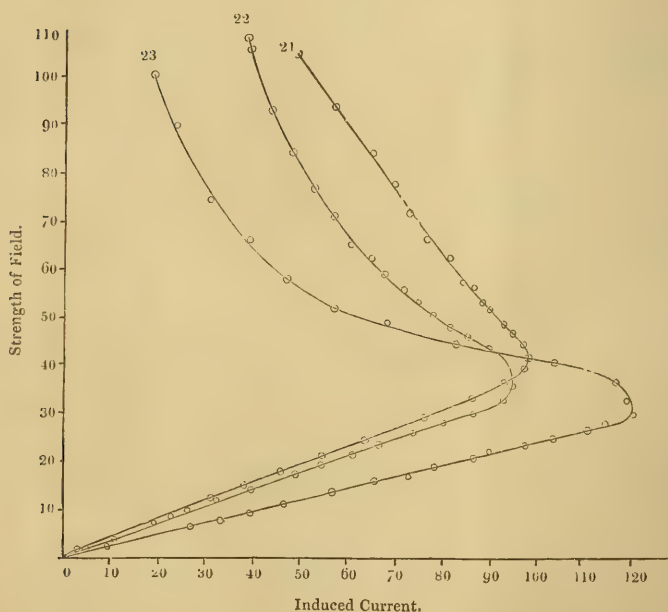
TABLE III.—Core, Untempered Soft Steel.—Diaphragm, Disk of Ferrotypes Iron, No. 31.

Strength of field.	Induced current.	Strength of field.	Induced current.
·031	8·2	·275	18·0
·049	12·0	·321	16·5
·089	22·0	·388	15·2
·137	25·9	·473	13·8
·157	24·9	·618	12·7
·171	22·9	·776	11·0
·194	21·1	1·011	9·5
·216	19·7		

An examination of fig. 1, as well as of the various curves following it, will show that the effect of increasing the strength of the magnet of the transmitter is in all cases to cause at first a rather rapid increase in the strength of the induced current, which later increases less rapidly, rising soon to a maximum value, from which it falls off, at first rapidly, and afterwards more and more slowly as the strength of the field is further increased. We proceed to consider the explanation of these results.

It is evident that three distinct sources of variation exist to affect the current furnished by a magneto-transmitter as the strength of the magnet is increased. First, the direct effect

Fig. 2.

Iron.

of the increased strength of field in which the diaphragm moves is to increase proportionally the strength of the induced current, since it increases correspondingly the rate of change in the number of lines of force enclosed by the coil of the instrument; second, an approach towards saturation of the

magnet, so far as it alone is concerned, will tend to diminish the induced current, on account of the smaller variation in the strength of the pole due to a given motion of the diaphragm; and, third, the nearer approach toward saturation of the diaphragm will have the same tendency.

The rapid rise in the induced current at the beginning is of course due to the predominating influence of the increasing strength of the field in which the diaphragm moves, as both core and diaphragm are then but slightly magnetized. The subsequent changes in the current must be explained by a consideration of the increasing magnetization of either the core or the diaphragm, or both.

It will be seen by comparing Tables I., II., and III., that the value of the maximum induced current for a given excursion of the diaphragm is approximately the same with all the three cores used, and that the same is true as to the strength of field corresponding to this maximum current. Moreover, saturation curves constructed for the several cores showed that in all cases the magnet was still very far removed even from half-saturation when the maximum induced current was obtained. From these facts it appears that the degree of saturation of the magnet is practically unimportant, so far as the general results shown in fig. 1 are concerned.

It remains to observe the part played by the increasing magnetization of the diaphragm. Other things remaining the same, as this approaches more and more closely towards saturation the increase in the number of lines of force passing between it and the magnet on the approach of the diaphragm to the magnet must become smaller and smaller; and this change will tend to oppose the effect of the increased absolute strength of the magnetizing force. The small mass of the diaphragm will evidently cause it to show the effect of an approach to saturation while the core is far below that condition. And such an action will clearly explain the observed changes in the current strength.

In order to test this matter still further, the experiment was tried of varying the mass and material of the diaphragm.

The results are shown by the curves in figs. 1, 2, and 3, which are constructed from the data given in Tables I. to X.; further results of the same character are given in Tables XI. and XII.

TABLE IV.

Core, Norway Iron.—Diaphragm, two superposed Disks of Ferrotypc Iron, No. 31.

Strength of field.	Induced current.	Strength of field.	Induced current.
·016	5·0	·184	49·3
·054	19·3	·205	50·2
·070	25·7	·229	48·0
·081	28·5	·266	40·3
·089	30·2	·306	31·8
·103	35·0	·342	27·7
·119	38·5	·433	16·7
·133	40·7	·530	11·9
·144	42·7	·667	8·3
·155	45·7	·830	7·0
·169	47·0	1·018	5·0

TABLE V.

Core, Norway Iron.—Diaphragm, three superposed Disks of Ferrotypc Iron, No. 31.

Strength of field.	Induced current.	Strength of field.	Induced current.
·000	0·5	·346	59·7
·018	3·2	·366	60·7
·047	8·0	·390	61·3
·079	14·2	·416	63·0
·084	14·7	·445	62·3
·096	17·0	·479	61·0
·110	19·7	·514	58·5
·118	21·8	·550	55·7
·135	25·5	·591	52·4
·151	28·3	·637	48·7
·175	33·0	·659	47·3
·213	39·0	·765	41·7
·249	46·3	·854	36·7
·277	49·8	·983	30·0
·313	55·3	1·180	22·8
·329	58·7		

TABLE VI.

Core, Norway Iron.—Diaphragm, Sheet Iron, No. 21.

Strength of field.	Induced current.	Strength of field.	Induced current.
.....	0.3	.456	96.7
.016	3.1	.477	94.0
.058	13.7	.499	92.5
.082	20.3	.521	90.5
.102	25.2	.536	88.3
.125	31.2	.573	86.7
.150	37.8	.584	84.0
.178	45.5	.637	81.0
.213	54.7	.675	76.7
.249	63.8	.724	72.3
.296	75.5	.784	69.3
.348	86.2	.851	65.6
.378	92.3	.949	57.3
.416	96.7	1.061	49.1
.437	97.8		

TABLE VII.

Core, Norway Iron.—Diaphragm, Sheet Iron, No. 22.

Strength of field.	Induced current.	Strength of field.	Induced current.
.....	1.7	.410	94.2
.018	4.7	.431	92.0
.047	11.7	.452	88.3
.075	19.7	.473	85.0
.088	22.3	.494	81.2
.100	26.0	.516	77.5
.123	32.5	.541	74.7
.146	39.5	.566	71.3
.171	48.2	.601	67.7
.194	54.2	.635	64.0
.218	61.3	.659	60.0
.240	66.9	.719	56.7
.262	73.2	.779	53.0
.292	81.3	.851	48.0
.313	87.7	.946	44.0
.342	92.7	1.072	39.3
.372	94.3	1.091	38.3

TABLE VIII.
Core, Norway Iron.—Diaphragm, Sheet Iron, No. 23.

Strength of field.	Induced current.	Strength of field.	Induced current.
.....	1·7	·272	109·3
·026	10·0	·289	113·8
·068	26·8	·310	119·3
·086	33·0	·339	118·6
·096	39·0	·382	114·1
·116	47·0	·420	102·9
·139	56·3	·458	82·2
·162	65·8	·499	68·7
·176	72·3	·529	57·6
·194	78·3	·582	48·0
·216	86·4	·652	39·9
·229	90·0	·751	30·8
·242	96·3	·904	23·7
·255	101·8	1·011	19·0

TABLE IX.
Core, Norway Iron.—Diaphragm, Steel, No. 26, Untempered.

Strength of field.	Induced current.	Strength of field.	Induced current.
·004	7·7	·281	66·7
·019	11·5	·313	63·8
·054	20·5	·342	60·4
·095	32·2	·380	56·3
·105	34·5	·422	50·5
·128	42·0	·483	44·7
·153	48·3	·530	39·0
·178	55·7	·584	35·3
·200	60·3	·652	32·0
·227	64·3	·810	28·2
·236	67·0	1·043	24·3

TABLE X.
Core, Norway Iron.—Diaphragm, Steel, No. 26, Tempered.

Strength of field.	Induced current.	Strength of field.	Induced current.
·002	7·8	·344	36·3
·019	10·4	·400	32·0
·068	18·5	·466	26·9
·096	23·8	·545	21·7
·128	30·3	·642	17·7
·151	32·4	·662	17·3
·180	37·0	·732	15·2
·214	40·2	·816	13·8
·246	40·0	·854	13·0
·272	39·5	1·025	10·0
·304	37·9		

TABLE XI.

Core, Norway Iron.—Diaphragm, Steel, No. 22, Untempered.

Strength of field.	Induced current.	Strength of field.	Induced current.
·007	6·2	·272	36·7
·023	8·0	·302	34·8
·070	17·3	·340	33·0
·103	23·0	·392	29·7
·121	26·4	·458	26·3
·139	30·1	·538	23·2
·157	32·8	·566	23·0
·176	35·0	·647	21·7
·202	37·1	·773	19·8
·224	34·9	1·046	17·6
·246	37·0		

TABLE XII.

Core, Norway Iron.—Diaphragm, Steel, No. 30, Tempered.

Strength of field.	Induced current.	Strength of field.	Induced current.
·007	6·2	·272	36·7
·023	8·0	·302	34·8
·070	17·3	·340	33·0
·103	23·0	·392	29·7
·121	26·4	·458	26·3
·139	30·1	·538	23·2
·157	32·8	·566	23·0
·176	35·0	·647	21·7
·202	37·1	·773	19·8
·224	1·046	17·6
·246	37·0		

TABLE XIII.

Core, Norway Iron.—Diaphragm, Steel, No. 30, Untempered.

Strength of field.	Induced current.	Strength of field.	Induced current.
·005	5·2	·331	31·2
·031	11·7	·376	28·9
·082	24·2	·437	26·0
·103	29·7	·512	23·3
·130	37·7	·632	20·4
·160	41·9	·735	19·7
·200	38·3	·819	18·0
·231	36·2	·956	16·3
·272	35·2		

Curve 2, fig. 1, represents the results when two of the ordinary ferrotype diaphragms were superposed, forming a diaphragm of double thickness, and curve 3 of the same figure shows the results when three such diaphragms were superposed. Each diaphragm was 0.01 of an inch thick. The curves of fig. 2, marked 21, 22, 23, respectively, show the results of similar experiments with diaphragms of sheet iron whose thickness was 0.030, 0.027, 0.024 of an inch, respectively (Nos. 21, 22, 23, B.W.G.). Fig. 3 shows the results when a steel diaphragm 0.017 of an inch thick (No. 26) was used, the curve *v* being that for untempered steel, and *r* that for tempered steel. Steel diaphragms respectively 0.026 and 0.012 of an inch thick (Nos. 22 and 30) gave similar results, as will be seen from Tables XI., XII., and XIII.

An inspection of these curves shows immediately that the greater the strength of the field required to saturate the diaphragm, the greater is the strength of the field at which the maximum current occurs. Thus in fig. 1 the maximum current with curve 1 corresponds to a strength of field of about 12 units of the scale used, while with curves 2 and 3 the corresponding strengths of field are 20 and 43 units respectively. Also in fig. 2 the maximum currents will be seen to correspond to greater strengths of field in proportion to the thickness of the diaphragm; and in fig. 3 similar though less marked results hold for tempered as compared with untempered steel of the same thickness.

It would also be expected that the value of the maximum current would be greater with a thick than with a thin diaphragm. This was usually the case in our experiments. Thus the curves 1, 2, 3 of fig. 1 give maximum currents of 27.5, 50.2, and 62.6 units respectively. Results of a similar nature are shown by fig. 3, the maximum current with the untempered diaphragm being far greater than with the tempered one. Curve No. 23 of fig. 2 is apparently an exception. It is probable, however, that this is in appearance only, and that the three curves of that figure are not strictly comparable with one another. The rigidity of the diaphragms here used, especially of the thicker ones, is considerable; and any slight yielding of the supports of the rod which carried the cam would prevent the actual throw of the diaphragm from being as great when this had considerable thickness, and would greatly diminish the strength of the current produced.

The peculiarity of curve 2, as compared with 1 and 3, is probably caused by the want of both magnetic and mechanical continuity in the material of the multiple plate formed by the several diaphragms used.

In those cases where steel diaphragms were employed, there was always a notable induced current, even when the reading of the magnetometer was zero. This was probably due to a slight residual magnetization of the diaphragms.

The results stated in this paper may serve to explain a phenomenon which has seemed somewhat obscure. Frequent attempts have been made to increase the efficiency of a magneto-transmitter by polarizing the diaphragm as well as the magnet; a common way of doing this being to employ a horseshoe-magnet, one leg of which is in contact with the edge of the diaphragm, while the other, about which the coil is wound, is placed in its usual position opposite the centre. But as a general rule little or no gain has seemed to result therefrom, so far as can be judged by the performance of such instruments in actual practice. It is quite probable in this case that the increased approach to saturation of the diaphragm may have so great an effect as entirely to prevent the expected improvement.

It will also be seen from our results that an increase in the thickness of the diaphragm of a magneto-transmitter tends to allow of the use of a stronger magnet, and for a given amplitude of vibration to produce a stronger current. But it must be remembered, on the other hand, that the greater rigidity of the thick diaphragm will diminish this range of vibration under the action of the voice, a difficulty which may to a certain extent be remedied by using a diaphragm of large diameter.

XLVIII. *On Diamagnetism and the Concentration of Energy.*
By J. PARKER, B.A., late Scholar of St. John's College,
Cambridge*.

MANY of the discoveries which have been made in Physical science in recent times may be classed under two great heads—the principle of the Conservation of Energy and Carnot's principle. These principles are closely related; the former being mainly an experimental conclusion, the latter a deduction from the first by means of Carnot's axiom. It follows, therefore, that whenever the principle of Energy ceases to hold, Carnot's principle will fail at the same time, but that the failure of Carnot's principle does not necessarily invalidate the principle of Energy.

Carnot's principle only holds when the material system which we are considering is restricted to receiving or losing energy from other systems in the forms of heat and mechanical work;

* Communicated by the Author.

but the passage of heat may take place either by conduction or radiation. The simplest case is obtained by supposing that there are only two external bodies with which the system can exchange heat. It was then assumed by Carnot that, in a complete cycle, it will be impossible, without an expenditure of mechanical work, to transfer heat by means of the system from the colder of the two bodies to the hotter. Mechanical work can therefore only be obtained from the system, during a complete cycle of operations, when it absorbs heat from the hotter of the two bodies and gives out heat to the colder. Consequently, if the energy of a material system consist entirely of heat of uniform temperature, it will be impossible to transform any of it into work.

Now it is found that all kinds of energy tend to pass into heat, and the passage of heat from a hot body to a colder (without the production of work) is an everyday occurrence. It has therefore been predicted with confidence that our universe is approaching a state in which the whole of its energy will be in the form of heat of uniform temperature, and all kinds of mechanical action impossible.

The following consideration, however, appears to offer a serious difficulty to the universal application of Carnot's principle. Thus, let A be a piece of permanently magnetized hard steel; and let B be a piece of a soft diamagnetic substance, as bismuth, which, when brought within the influence of A, becomes magnetized by induction and is *repelled* by A. Then suppose that the following cycles of operations are performed at constant temperature:—

(a) Let B be removed from a position P, remote from A, to a second position Q, near A, so slowly that at every instant the magnetization of B has its maximum value; and let the work expended be called W. Then let B return slowly to its original position P by the former path reversed. The work W, which had been expended, will be recovered; so that, on the whole, there will be neither gain nor loss of mechanical work.

(b) Let B be removed from P to Q so rapidly that the magnetization of B has not time to alter sensibly. The work done on B will be less than W. After allowing B to remain long enough in the position Q to attain its permanent magnetic state, let it return rapidly from Q to P by the first path reversed. The work restored by B will be greater than W. There is therefore a gain of work in this cycle performed at constant temperature, contrary to Carnot's principle.

There are three ways of looking at this difficulty:—

(1) We may suppose that the work which has been obtained

has been created from nothing. This would involve a contradiction both of the principle of Energy and of Carnot's principle, and is the view generally held at present.

(2) The development of magnetism in diamagnetic bodies may be instantaneous, unlike all other physical phenomena, which require *time*.

(3) The work which has been gained may have been produced from *heat*; so that the principle of Energy stands, while Carnot's principle falls. Employing this work to transfer heat from a cold body to a hotter, we have a means of producing inequalities of temperature—that is, a Concentration of Energy—without external assistance. Carnot's principle will then require to be modified.

It has been shown by Clausius that for any cycle which satisfies Carnot's principle in its usual form, we have $\int \frac{Q}{t} = 0$ if the cycle be reversible, and $\int \frac{Q}{t} < 0$ in other cases, Q being the heat absorbed when the absolute temperature is t . It seems probable that these results may be true for soft paramagnetic bodies, but that for diamagnetic bodies we should have $\int \frac{Q}{t} = 0$ for a reversible cycle, and $\int \frac{Q}{t} > 0$ in other cases. We might then obtain expressions for the energy and entropy of a magnetized system, and a thermodynamical theory could be formed for Magnetism as easily as for Electricity.

XLIX. *On the probable Cause of the Displacement of Shore-lines, an Attempt at a Geological Chronology.* By A. BLYTT*. *Norwegian.*

THIS memoir is an attempt to further develop and establish ideas which I put forward five years ago. It contains an attempt to establish a chronology in geology. It sets forth what the English call "a working hypothesis," without claiming to be anything else. It was the distribution of plants which first introduced the author to this great question; but the problem of a chronology in geology cannot be solved without the co-operation, it may perhaps be said, of all naturalists. It certainly cannot finally be solved by any one man. In putting forth my hypothesis I must in the

* Read at the General Meetings of the Society of Science of Christiania, December 9, 1887, and June 1, 1888. Translated by W. S. Dallas, F.L.S., from the *Nyt Magazin for Naturvidenskaberne*, Bd. xxxi. pp. 240-297 (1889).

first place beg for indulgence for the many faults and imperfections with which such an attempt must be affected, and express a hope that in any case the hypothesis may be found worthy of being further tested.

Having endeavoured in several memoirs on the distribution of plants, on peat-mosses, shore-lines, terraces, and morainic ridges, to show that climates undergo periodical changes, I published in the Transactions of the Society of Sciences for 1883 (No. 9) a memoir on Alternation of Strata and its possible significance for the chronology of geology and the theory of the modification of species. The essential contents of this paper, as regards the present question of geological chronology, were as follows :—

Alternation of strata, under which term is understood an alternation of geological formations of different constitution, can be produced by local conditions of rapidly passing change, without the action of general and persistent causes. But there are also causes of the latter kind which effect an alternation of the strata. Two such periodically acting causes are traceable in the geological series of deposits—a shorter, somewhat regular one, and a longer, more irregular one. The former effects a change of climate, the strength of the marine currents alternately diminishing and increasing during thousands of years ; the latter, longer period effects a rise or fall of the sea in relation to the land, and an alternation of deep-sea formations with shore-formations or fresh-water deposits. The opinion has been expressed that these periods, which are traced in the series of deposits, might possibly stand in connexion with the two cosmical periods revealed by Astronomy—the precession of the equinoxial points, and variations in the eccentricity of the earth's orbit ; although in the memoir referred to it is not attempted to show in what manner such a connexion could be established. But if, with the aid of these two hypotheses, we construct an “artificial” series of strata, we find that one with no less than 46 changes of deposit may be recognized, bed by bed, in the Tertiary formations of the Paris basin.

This result may encourage us to test still further the correctness of the two suppositions. As regards the precession this has been attempted in my paper “On the probable Cause of the Periodical Change in the Strength of the Marine Currents.” *

* *Vid. Selsk. Förh. Christiania*, 14 December, 1883 ; *Archiv f. Math. og Naturv.* ix. Christiania, 1884.

The contents of this memoir are essentially as follows :— The precession of the equinoxial lines causes the summers in about 10,500 years to be longer, and in the following 10,500 years shorter, than the winters. The conditions are opposite in the northern and southern hemispheres. The difference between the number of winter and summer days increases with the eccentricity of the earth's orbit.

The cooling of continents under high latitudes in the winter produces a diminished pressure of air over the sea. This low pressure draws air from lower latitudes. For this reason, in the Atlantic south-west winds prevail. Thus, in the winter, the south-west winds of the North Atlantic are on an average three times as strong as in the summer, in consequence of the great refrigeration of the mainland. In the semi-period when the winter falls in aphelion the average annual wind-force is consequently greater. Now it is the prevalent wind that produces the powerful marine currents, such as the warm current in the Atlantic Ocean. The strength of the marine currents is dependent upon the average wind-force for the last great time-period. Now, as this average wind-force is periodically variable in consequence of the precessions, the strength of marine currents and the temperature of the sea must also be subject to a periodical variability. For about 10,500 years the warm sea-current will increase, to diminish in the next similar period, and so on constantly through all time. When the winter falls in aphelion, the difference between the littoral and inland climates will increase. The propelling force of currents in the sea will increase and diminish by 1–5 per cent. upon their total annual value according as the winter falls in aphelion or perihelion, and according as the eccentricity of the earth's orbit is small or great.

Such an alteration in the strength of the marine currents will produce an alteration of the climate, which, however, will not be very important, but which will nevertheless be great enough to leave its traces in the deposits. During colder and drier seasons the streams are fed in great part by spring water. This water has drained slowly through the beds and is charged with dissolved materials ; but the small quantity of water and the feeble stream carries less clay, sand, and gravel. During rainy seasons the rain carries down quantities of such materials, but it flows off rapidly, and, as it for the most part runs only over the surface, it has not time to dissolve so much. Although the springs flow more abundantly during rainy seasons, their water only mingles with the rain-water. The streams are therefore poorer in dissolved material,

but they contain more water, and their more powerful current carries more clay, sand, and gravel into the basin. Hence the drier seasons will be richer in purely chemical deposits, which will be transported in the clearer water; the wet seasons in mechanical deposits. Strata of both kinds are formed, of course, at all times, but they are deposited at different places in accordance with the variation in the quantity of rain. Thus, I assume that when thick deposits of river-sand and clay alternate with each other, when soft clay and marl alternate with hard marl or limestone, when thick strata of loose sand alternate with sandstone, which is bound together by chemically produced cement (iron, silica, lime), when clay alternates with *Septaria*-beds &c., then, in each case, the first-named deposit shows itself to belong to seasons with a warmer sea and a greater quantity of rain, which, as regards Western Europe, will mean seasons with the winter in aphelion.

That this alternation of deposits implies a period of several thousand years' duration is shown by the fact that the fossils change rapidly through the strata. In the Tertiary formations there are only a few, often only 4-5, such changes of deposits in each stage. The whole Oligocene period has only about 30, the Miocene still fewer, and the Pliocene barely 20 such changes.

In this way, in my opinion, the precessions stamp themselves upon the strata, and this should therefore furnish a means of measuring time. The greater the eccentricity of the orbit, the more strongly marked will the periods be; when the orbit approaches the circular form, they are less recognizable*.

Referring for other things to the two memoirs cited and to my paper "*On Variations of the Weather in the course of Time*" (*Letterstedtske Nordisk Tidsskrift*, 1885, in English in *Forh. Vid. Selsk. i Christiania*, 1886, No. 8) I will pass on to examine whether there is any probable ground for supposing that the other proposition is also correct, whether it is con-

* But the perihelion also shifts to and fro. The time between two aphelia in the winter solstice varied thus in postglacial times by fully 4600 years. This must have some influence. The longer a period with winters in aphelion lasts the longer will the warm currents in the Atlantic increase in strength, and the greater will be the changes of climate. The mild period during which Bergenian sea-animals lived in the Christiania Fjord, and which has left its traces elsewhere in our hemisphere, was, in my opinion, a consequence of such an unusually long period with the winter in aphelion. The winter solstice fell in aphelion (according to Croll) 61,300, 33,300, and 11,700 years ago. The middle of the Atlantic period with Bergenian sea-animals in the Christiania Fjord fell, from the testimony of the peat-mosses, 33-34,000 years ago, therefore in accordance with the period of 28,000 years.

ceivable that under high latitudes the sea-level rises and sinks with the eccentricity of the earth's orbit.

Great part of the earth's surface consists of strata which still lie undisturbed in their original horizontal position. These parts are called "tables" by Suess. But in many places the crust of the earth is so traversed by clefts and fissures that it may be compared to a breccia. Fragments are often displaced relatively by thousands of feet. Strata which originally lay horizontally are folded, thicknesses of 7000-8000 feet are bent as if they were straws (Kjerulf, *Udsigt over Norges Geologi*, 1879, p. 76). Moreover, the folded strata are upheaved far above their original level. Even marine formations so recent as the Eocene are uplifted to heights of 21,000 feet above the sea (Suess, *Antlitz der Erde*, i. p. 564). Sometimes they stand vertically, or are inverted, so that older strata cover the younger ones. Through fissures eruptive masses are brought forth, and have covered thousands upon thousands of square kilometres. The distribution of land and sea also varies. It is indeed supposed that the great depths of the ocean and the great continents have essentially retained their original distribution from the most ancient times, but the shore-lines wander periodically to and fro; and these changes of the earth's surface have taken place from earliest times, and are still in action at the present day.

Geologists in general seek the explanation of these phenomena in the cooling and contraction of the body of the earth. The earth's crust folds, just as the skin of an apple wrinkles as the apple dries. The leading geologists of the present day adopt this theory, and A. Geikie in his 'Text-book of Geology' (London, 1882, p. 287) says with perfect justice:—"With modifications, the main cause of terrestrial movements is still sought in secular contraction."

According to this doctrine changes in the crust of the earth are due to the interior contracting more strongly than "the crust," so that the latter is too large for it. Its weight drags it down. By this means great horizontally acting pressure is produced in the crust, which must then become folded and cracked in places. The fragments sink down. By this means are formed what Suess has called "Einbrüche." When a part of the crust remains in position while all around it sinks, there is produced what Suess has called a "Horst." The old theory of forces acting vertically from below is most decidedly rejected by Suess. He and Heim have shown, by their investigations of the Alps, that the foldings of the Alps

are caused by lateral pressure, and that such lateral pressure is sufficient to lift great chains of mountains into the air. But Suess goes still further, for in a memoir, "Ueber die vermeintlichen säcularen Schwankungen einzelner Theile der Erdoberfläche" (in *Verh. K. K. Geol. Reichs.* 1880, pp. 171 *et seqq.*) he even denies any elevation by forces acting vertically from below—neither mountain nor continent is elevated in this manner. He says (*l. c.* p. 180):—"There are no vertical movements of the solid ground, with the exception of those which proceed directly from the formation of folds. We shall have to resolve to abandon the doctrine of the secular oscillations of continents."

A. de Lapparent, who sharply criticises Suess's theory of "Horste" (*Bull. Soc. Géol. France*, sér. 3, tome xv. pp. 215 *et seqq.*), nevertheless agrees with him that the cooling of the earth has formed great folds in the crust, and denies that any elevations are not caused by foldings. Thus he says (*l. c.* p. 217):—"It is no longer necessary to oppose to the doctrine of absolute elevations produced by forces acting directly from below upwards, a protestation which has lost its object. For the partizans of vertical impulsions are nowadays more than scattered, and with the exception of a very few belated persons no one would now venture to ascribe to such an action an important part in the formation of mountains." As he makes no limitation, it must be assumed that he will not recognize any forces acting from below to elevate whole land-masses.

According to a statement of Suess's, in his *Antlitz der Erde* (1885, Bd. i. p. 741), he seems to find an essential reason for denying elevation by forces acting perpendicularly from below in that we are quite ignorant of any force which could be capable of causing such an elevation.

The theories of Hutton and von Buch as to the action of such forces seem therefore to be rejected by geologists of the present day. Nevertheless there are still a few who hold similar opinions. Thus J. C. Russel (U.S. Geol. Surv. 4th Ann. Report, Washington, 1884, pp. 452, 453) says that the fractures in "the Great Basin" are not in consequence of any lateral pressure, but are caused by an extension in a horizontal direction:—"The fractures are closely related to an extension of the strata caused by upheaval." It seems to me improbable that such a relation should be explicable by a folding. C. E. Dutton also (U.S. Geol. Surv. 6th Ann. Report, 1885, p. 198), at the same time that he recognizes that many chains are folded by lateral pressure, says, with regard to the mountain-masses in Western North America:—"The

mountains of the West have not been produced by horizontal compression, but by some unknown forces beneath which have pushed them up”*.

It is not my intention to maintain that refrigeration has not at all contributed to give the surface of the earth the form which it now possesses. But I think that an auxiliary theory is required, which, while it will not entirely supersede the old theory, may yet serve to explain things which the old theory cannot render comprehensible.

Henry H. Howorth has written two memoirs, namely, “Recent Elevations of the Earth’s Surface in the Northern Circumpolar Regions” (Journ. Roy. Geogr. Soc. vol. xliii. 1873, p. 240) and “Recent Changes in the Southern Circumpolar Regions” (*op. cit.* vol. xliv. 1874, p. 252), in which he has brought together what was at that time known as to the displacement of shore-lines in the last section of geological time, and the principal result of his investigations is summed up in the following words:—“The South Pole, as well as the North, is a focus of protrusion, the land around it is being gradually elevated.” In the last section of geological time, *i. e.* in the Postglacial period, the land has, in general, sunk under lower and risen under higher latitudes.

Suess arrives at a similar result in his above-cited memoir (*Verh. K. K. Geol. Reichs.* 1880, pp. 174–175). He has likewise studied the displacement of coast-lines over the whole earth during the period nearest to the present time, and sums up the result as follows:—“*Terraced land* [*i. e.* land which has recently risen in relation to the sea] *appears everywhere in the high northern latitudes*, so far as man has hitherto penetrated into these solitudes. It also extends far, although not everywhere equally far, down into the temperate latitudes, but generally decreasing in height. In other words, around the North Pole and far down the sum of the negative [*i. e.* descending] movements of the coast-lines is greater than the positive; towards the south, however, these two sums approximate more and more. In *tropical seas*, in the regions of the coral forma-

* The current doctrines with regard to refrigeration and compression are discussed by Peirce in a discourse before the American Academy on the 11th May, 1869 (see Proc. Amer. Acad. Arts and Sci. vol. viii. 1873, p. 106), as also by O. Fisher (Physics of the Earth’s Crust, 1881) and Dutton (“A criticism upon the Contractual Hypothesis,” in Amer. Journ. Sci. ser. 3, vol. viii. 1874, pp. 113 *et seqq.*). They all consider that contraction is not sufficient to explain the known phenomena; nay, the last-named even thinks that the phenomena are opposed to this. A. de Lapparent, on the other hand, in his memoir “Contraction et refroidissement du globe” (*Bull. Soc. Géol. France*, sér. 3, vol. xv. 1887, pp. 383 *et seqq.*) seeks to prove that they are quite sufficient.

tions, *the opposite condition occurs*, the sum of the positive movements preponderates. *Further towards the south*, beyond 25° – 35° south latitude, *the terraced land of the north begins again in South America, South Africa, South Australia, and New Zealand, i. e.* the same preponderance of the negative movements, with the same oscillating* character as in the north." The exceptions (according to Suess) are few and of little importance.

Howorth and Suess have therefore both come to the same result. But their explanations are directly opposite. Howorth thinks that it is the land which has risen under higher latitudes; that the earth, as it were, swells up towards the poles and contracts under the tropics. Suess, who will not admit any other elevations than those which are the consequences of foldings, is of opinion that it is the sea which has flowed towards the lower latitudes. He indicates as a possible explanation changes in the length of the day and the centrifugal force. But this change should then only have acted upon the sea, and therefore, since the sea has flowed towards the equator, the day should have been considerably shorter in the last geological period. We shall see hereafter that there is no known cause which could have produced such a shortening of the Sidereal day as would serve to explain what Suess wants to explain. The old theory of refrigeration is scarcely fitted to explain these conditions indicated by Howorth and Suess. Even Suess, who is a zealous adherent of the theory of contraction, is obliged here to seek for another explanation.

Another theory, however, has come forth in our day, a theory which, no doubt, is destined to play a great part in geology. It is derived originally from the celebrated philosopher J. Kant. In 1754 he wrote a memoir entitled "*Untersuchung der Frage: ob die Erde eine Veränderung ihrer Achsendrehung erlitten habe?*" In this it is shown that, by reason of the attraction of the moon and sun, the sea is constantly in a movement opposite to the daily revolution of the earth. The friction of the tidal waves against the bottom and coasts of the sea diminishes the force of the axial revolution and works constantly in the same direction, so that the sidereal days must for this reason always become longer and longer. The moon always turns the same side

* With this word Suess alludes to the circumstance that the coast-lines and terraces occur at various levels one above the other. He thinks that each of these levels indicates an oscillation of the sea. I believe that *the greater part* of these levels are merely a consequence of climatic changes due to the precessions. (See *Forh. Vid. Selsk. Christ.* 1881, No. 4.)

towards the earth because the earth's tidal action on the mass of the moon while still fluid constantly rendered the axial revolution of the moon slower; until at last the moon was compelled to turn always the same side towards the earth*. In this way also, at some far distant period, the earth will come to turn the same side always to the moon. This opinion of Kant's has been recognized as correct by the first physicists of the present day, by men such as Robert Mayer, Helmholtz, and W. Thomson.

There are certain peculiarities in the moon's movements which astronomers are inclined to explain by the assumption that the sidereal day gradually increases by reason of the friction of the tidal wave. But with regard to this we will merely refer the reader to Thomson and Tait's 'Treatise on Natural Philosophy,' and to a memoir by the first-named author, "On Geological Time" (Trans. Geol. Soc. Glasgow, vol. iii. 1868, pp. 1 *et seqq.*).

In their 'Natural Philosophy,' Thomson and Tait treat the problem of the earth's axial rotation. They state that there are various forces which may be efficient in altering it—some make the sidereal day shorter, others make it longer. The latter are preponderant, and among them, again, the tidal wave plays the greatest part; so that for this reason in the course of time the sidereal day becomes always longer and longer. Refrigeration is the most powerful force which contributes towards the shortening of the sidereal day, but its action is calculated by Thomson (Trans. Geol. Soc. Glasgow, *l. c.* p. 28) at only $\frac{1}{6000}$ of the tidal wave; and this last action cannot be annulled by any of the other forces, which act sometimes in one, sometimes in another direction (transport of material from higher to lower latitudes, or *vice versâ*, accumulation of ice at the poles, &c.), and which in course of time cease to act, the tidal wave acting always, for millions of years, in the same direction (Thomson, "Geological Dynamics," Trans. Geol. Soc. Glasgow, vol. iii. part 2, 1869, p. 223).

In this way, therefore, the sidereal day must in course of time always become longer and longer. Now, what influence has this upon the earth? If this were fluid throughout, it is clear that it must at once change its form. According as the sidereal day became longer and the centrifugal force diminished, its compression must have decreased. But the old theory of a fiery fluid interior is now rejected by physicists,

* Is it possible that the great abundance of old volcanoes in the moon may be explained by the great change which its axial rotation, and therefore probably also its compression, has undergone?—A. B.

and Thomson assumes that the earth is on the whole a solid body. Now will this solid body retain its form without reference to the length of the sidereal day, or will it yield and accommodate itself? The sea, as a matter of course, will at once yield, and, as the centrifugal force decreases, it will sink under lower and rise under higher latitudes. We know that the earth's present form agrees, at all events in some degree, with the length of the sidereal day. It has at present a compression which about agrees with that which it should have from calculation, with its present axial rotation. As it may now be rendered probable that the earth, since it acquired a solid surface, has lost so much of its axial rotation that the sidereal day has become several times longer, the circumstance that the compression suits that agreeing with the axial rotation seems to show that the solid earth has really changed its form. Jupiter and Saturn have a sidereal day respectively of 9 h. 55 m. and 10 h. 15 m., and a compression of $\frac{1}{17}$ and $\frac{1}{10}$. In Mars, the sidereal day of which is about 24 h. 37 m., observations have not been able to prove definitely any compression. There would seem, therefore, to be a connexion between compression and axial rotation. But it may indeed be objected that Jupiter and Saturn are still possibly melted masses.

W. Thomson and Tait seem to be of opinion that the earth will not change its form. They assume that it must have become solid not so many millions of years since, seeing that the compression nearly coincides with the axial rotation.

J. Croll ('Climate and Time,' 1875, p. 335; see also Amer. Journ. Sci. ser. 3, vol. xii. 1876, p. 457) thinks that the sidereal day lengthens so slowly that denudation will have time to adjust the form of the earth so as to coincide with the length of the sidereal day. Just as the sea sinks under low latitudes, the continents in the same latitudes will also become lower by denudation, but under higher latitudes the rising sea will protect the land instead of denuding it; and in this way the earth must then, by denudation alone, acquire a form always suitable to its axial rotation. But this is evidently erroneous. Imagine the earth formed of ellipsoidal layers with increasing solidity inwards. When the centrifugal force diminished, equilibrium would be disturbed throughout the whole mass, and in the interior tension would constantly increase. Nay, not even at the surface can denudation alter the compression. For we know from the recent investigations of the deep sea, that in this deep sea, far from the continents, no products of weathering are present: only volcanic ashes and cosmical dust are deposited. Thus denudation is

not even capable of obliterating the inequalities of the surface, still less the internal tension produced by the lengthening of the sidereal day. And as the day has become considerably longer, the sea ought to be collected towards the poles and the land under the equator, in case the solid earth had not changed its form.

Others think that the earth may actually change its form. The first who expressed this opinion, so far as I can find, is Herbert Spencer. In the *Philosophical Magazine* (1847, vol. xxx. p. 194) he published a small memoir, entitled "The Form of the Earth no proof of original Fluidity," in which he maintains that even the solid earth may change its form, according as the centrifugal force changes. When a body increases in size, the power of resistance to external forces increases only as the square of the dimensions, while the wasting and destructive forces (weight, centrifugal force) increase in the same proportion as the mass of the body, and therefore as the cube of the dimensions. As the size increases we therefore come to a point at which even the most solid body must yield to the forces. We must therefore assume, says Spencer, that the earth, by reason of its size, must yield and change its form, in case the centrifugal force, for example, changes; for the most solid matter known to us, exposed to the same forces which act upon the earth, would overstep the bounds of solidity before attaining a thousand-millionth part of the earth's size. This argument, in Prof. Schiøtz's opinion, is not tenable. At any rate, I believe that Spencer is the first who expressed the opinion that even a solid earth can change its form. In the above-cited discourse of 1869, Peirce says that the lengthening of the sidereal day may be supposed to have altered the form of the solid earth. And Principal Dawson, in his 'Story of the Earth and Man' (ed. 9, 1887, p. 291), says that this alteration of form by reason of the lengthening of the sidereal day must have taken place at longer or shorter intervals. So long as the crust of the earth did not yield, the sea will have flowed towards the poles; but when the tension becomes so great that the solid crust bursts, the equatorial regions will sink in and the sea will flow again towards the equator*.

* Similar opinions are expressed by Dr. E. Reyer ("Die Bewegung im Festen," in *Jahrb. K. K. Geol. Reichs. Wien*, vol. xxx. 1880, pp. 543 *et seqq.*). W. B. Taylor, in a memoir "On the Crumpling of the Earth's Crust" (*Amer. Journ. Sci.* ser. 3, vol. xxx. 1885, pp. 249 *et seqq.*), expresses himself against the theory of the earth's contraction, and thinks that the lengthening of the sidereal day is the cause of the changes in the crust. A. Winchell, in a memoir on the "Sources of Trend and Crustal Surplusage" (*Amer. Journ. l. c.* p. 417), endeavours to show that the

In the Philosophical Transactions for 1879, Parts I. & II., Prof. G. Darwin has published a memoir the results of which are briefly as follows. He assumes that the earth possesses a small degree of plasticity, and calculates the internal friction which the tidal action of the moon and sun produce in such a body. He finds that both the sidereal day and the month have become much longer, that the distance of the moon has increased, that the obliquity of the ecliptic has diminished, and that a great part of the internal heat is developed by the internal friction. 46,300,000 years ago, according to his calculation, the sidereal day was 15 h. 30 m., and the moon's distance 46·8 terrestrial radii (against 60·4 at present). But 56,180,000 years ago the sidereal day was only 6 h. 45 m. long, the moon's distance only 9 terrestrial radii, and the month only 1·58 day ($\frac{1}{17}$ of its present amount). The interior heat produced by friction in 57,000,000 years, if applied at once, would suffice to heat the whole earth 1700° Fahr.* He concludes that the compression has constantly diminished:—*“the polar regions must have been ever rising, and the equatorial ones falling,* though as the ocean followed these changes they might quite well have left no geological traces†. The tides must have been very much more frequent and larger, and accordingly the rate of oceanic denudation much accelerated. The more rapid alternation of day and night [57,000,000 years ago, according to Darwin, the year had 1300 days] would probably lead to more sudden and violent storms; and the increased rotation of the earth would augment the violence of the trade-winds, which, in their turn, would affect oceanic currents‡.

Tresca (*Comptes Rendus*, 1864, p. 754; 1867, p. 802, &c.)

diminishing centrifugal force has produced foldings in a north and south direction. J. E. Todd, in a paper entitled “Geological Effects of a varying Rotation of the Earth” (*Amer. Naturalist*, vol. xvii. 1883, pp. 15 *et seqq.*), first enumerates the various forces which may act in accelerating and retarding the axial rotation. He assumes that the axial rotation decreases and increases abruptly, that it acts first upon the sea and afterwards upon the solid crust, and that for this reason the sea rises and sinks abruptly in relation to the land.

* This heat, produced by the internal friction, must contribute considerably to diminish the secular refrigeration. Lapparent has not taken account of this in the above-cited memoir on the contraction and cooling of the earth.

† In a subsequent article, however, Darwin supposes that the coast-lines will shift in consequence of the lengthening of the sidereal day (*‘Nature,’* Sept. 2, 1886, p. 422).

‡ The numerical values given above make no claim to represent the actual values; they are merely the maximum values, which, according to Darwin, are generally possible.

has shown that ice, lead, and also cast iron, even at ordinary temperatures, may be squeezed so strongly that their interior parts change their relative positions like particles in a fluid. Iron, in the solid state, by strong pressure, is squeezed into cavities and adapts its form to the surroundings. On cutting through such pressed pieces it has been found that the particles or crystals have arranged themselves by a flow-like movement suited to the form of the cavity into which the piece has been pressed.

We must here also refer to the interesting investigations of Reusch upon pressed conglomerates. Under the strong pressure which has acted in the earth's crust, the pebbles in conglomerates are squeezed out into lance-shaped bodies, and these bodies have even become folded. (See Reusch, *Silurfossiler og pressede Konglomerater i Bergensskiferne*, Univ. Progr. Christiania, 1882, pp. 15, 117.)

By reason of the enormous pressure which prevails in the interior of the earth, it must be supposed that masses from a certain depth are more or less in a plastic state. A constant lengthening of the sidereal day will cause the equatorial parts to increase in weight. So long as the earth does not change its form, a constantly increasing weight will act upon the internal mass from lower towards higher latitudes. There is, as Darwin indicates ('Nature,' Sept. 2, 1886, p. 422), reason to believe that, finally, when the tension has reached a certain amount, the earth will yield. A flow of plastic mass will be directed towards higher latitudes, and persist until the earth has approximated to the form suitable to the length of the sidereal day. When we consider the numerous testimonies as to changes in the solid crust of the earth, and the frequent elevations and depressions of the solid land relatively to the sea, we may well agree with Darwin that this view may claim more probability than that of Thomson and Tait.

Wertheim has proved by experiment (according to Fock, *Lärobok i Fysiken*, Stockholm, 1861, pp. 202, 219) that there is really no definite limit of elasticity for any matter, but that they all, by the action even of quite feeble forces, undergo small persistent changes, especially if these forces have acted for a somewhat long time. When with feeble pressures we find no permanent change of form, this is because the force has not acted long enough. The action of the force, therefore, when it has a greater resistance to overcome, depends upon time. "By tension," says Schiøtz (*Lærebog i Fysik*, Christiania, 1881, p. 65), "lengthening constantly increases, although very slowly, after it first commences; therefore a weight which has acted for a short time will not produce persistent elonga-

tion, such as it would if it were allowed to act for a longer time. This applies not only to tension, but generally; and hence it comes about that wires slacken in course of time, and that beams bend little by little. A thread is worn out by less force when the pressure is long continued than when it is applied for a shorter time."

It seems to me that here we have a force which may be capable of effecting displacements in the solid earth. I believe that this is "the unknown force from below" which has elevated the mountains of Western North America, and to which Dutton appeals. The sidereal day increases very slowly. The sea adjusts itself in accordance with the smallest change in the length of the day, and rises slowly under high latitudes. But the solid earth offers resistance to change of form, and begins to give way only when the tension reaches a certain amount. When this period has arrived the crust also begins to rise under high latitudes. Under lower latitudes the movement takes place in the opposite direction. The solid earth probably is a little behind the sea in its movements; and while the sea moves evenly and uninterruptedly, the change of form in the solid earth must perhaps take place more spasmodically, with intervening periods of rest, during which new tension is set up.

"The elevation of mountains," says A. Geikie ('Text-book of Geology,' 1882, p. 917), "is in most cases due to a long succession of such movements;" and (*l. c.* p. 919) "the elevation of mountains, like that of continents, has been occasional, and, so to speak, paroxysmal." Upheavals of the crust take place repeatedly along the same fissure (see, *e. g.*, Brögger, *Bildungsgeschichte des Kristianiafjords*, 1886, p. 78). Something of the same kind occurs in volcanic eruptions. Volcanoes rest for a shorter or longer time between the different eruptions. Basaltic layers alternate with sedimentary deposits. Earthquakes are a consequence of a tension set up, to which the crust suddenly yields. All this indicates that the crust of the earth does not immediately accommodate itself to the forces, but that it yields only when the constantly increasing pressure has approximated to a certain amount. It seems, moreover, to follow from geological investigations that there are periods in the earth's history when changes have taken place on a larger scale than usual. In his 'Text-book' above cited (pp. 197-198) A. Geikie refers to the great eruptions ("fissure-eruptions") which have taken place, in both the Old and the New World, in which melted masses burst forth from numerous fissures and overflowed thousands

of square miles. The Vulcanism of the present day seems feeble in comparison with these gigantic eruptions.

We will now pass to the inquiry whether these changes in the form of the earth may stand in any relation of dependency to the periodical variations of the eccentricity of the earth's orbit. We start from the fact that Thomson and Tait are right when they say that the tidal wave is the most powerful of the forces which contribute to change the length of the day. But besides the tidal wave of the sea, the interior friction accepted by Darwin, ("the bodily tides") is also effective. Both, of course, are dependent upon the distance of the sun and moon; and we may therefore examine whether the tidal action of these bodies upon the earth varies with the eccentricity of the earth's orbit. It appears from Darwin's investigations that the lunar tides in very distant periods must have been much greater than now. I disregard this, as the time in question is so long ago, and because the profiles, which later on will combine in curves for the eccentricity of the earth's orbit, come down from a past geologically so near. When I perceived that the dependence of the tidal wave upon the eccentricity might be of geological importance, I applied to the observer H. Geelmuyden, who, with his usual kindness, has given me the following answer:—

"The action of the eccentricity of the earth's orbit, e , upon the force which produces tide and ebb, and which, for the sake of brevity, I will call the tidal force, is as follows:—Let r be the sun's distance, then the sun's tidal force is

$$P = \frac{C}{r^3},$$

where C represents the sun's mass and the earth's radius. In the course of the year r varies; but the mean value of $\frac{1}{r^3}$ is found by a simple integration to be $\frac{1}{a^3(1-e^2)^{3/2}}$, where a is the unchangeable mean distance. Consequently, the annual mean value of the sun's tidal force becomes

$$P = \frac{C}{a^3(1-e^2)^{3/2}} = \frac{C}{a^3}(1 + 3/2 e + \dots).$$

"From this it follows that, when the eccentricity increases, the tidal force also increases; if the former increases Δe and the latter ΔP , then

$$\frac{\Delta P}{P} = \frac{3e \cdot \Delta e}{1-e^2} = 3e \cdot \Delta e,$$

as $1-e^2$ in the denominator is of no significance. If past times be $3e = \frac{1}{20}$, and $\Delta e = -0.00043$ per thousand years, then $3e \cdot \Delta e = -0.00002$, or the sun's tidal force decreases for every thousand years by $\frac{1}{50000}$ of its value. When the eccentricity has its greatest possible value, 0.0667 according to Leverrier, $e^2 = 0.00445$, $3/2 e^2 = 0.00667$, then $P = 1.00667 \frac{C}{a^3}$; or the difference between maximum and minimum is $\frac{1}{150}$ of the value.

"The monthly mean value of the moon's tidal force will of course, in the same way, be dependent upon the eccentricity of the moon's orbit; but as this is not subject to any noticeable secular variation, it does not come under consideration. On the other hand, the moon's mean distance is dependent, although only to an extremely small extent, upon the eccentricity of the earth's orbit, namely so that the moon's tidal force becomes

$$P' = \frac{C'}{a'^3} (1 - q \cdot 3/2 e^2).$$

"Here, therefore, the eccentricity acts in the opposite direction, namely so that the force diminishes as the eccentricity increases; but as the factor q , by which $3/2 e^2$ is multiplied, is only about $3/400$, while the magnitude outside the brackets $\frac{C'}{a'^3} = 5/2 \cdot \frac{C}{a^3}$ (the lunar tides being in proportion to the solar tides most nearly as $5:2$), its action upon the whole tidal wave is $\frac{3}{400} \cdot \frac{5}{2} = \frac{1}{160}$ of the former."

Thus we see that the tidal force rises and sinks with the eccentricity of the earth's orbit. It varies by about $\frac{1}{525}$ of its value from the highest to the lowest eccentricity. This force is the most important force for the alteration of the day, and it makes it longer. The most important force for shortening the day, according to Thomson, will be the refrigeration of the earth, but he has calculated its value at only $\frac{1}{6000}$ of the tidal force (and he has only taken into account the marine tidal wave). If, therefore, the tidal force diminishes and increases by $\frac{1}{525}$ of its value, this periodical variation cannot compete with forces which act in the opposite direction; and we may therefore conclude that the sidereal day is constantly becoming longer, but that its increase is periodically stronger and weaker. It increases in length more and more rapidly so long as the eccentricity of the earth's orbit increases, more and more slowly so long as the eccentricity diminishes. In other words: the centrifugal force diminishes and the equatorial regions increase in weight more and more rapidly

under an increasing, and more and more slowly under a diminishing eccentricity.

As has been stated, there prevails, even among physicists, a disagreement as to how far the earth will change its form, in case the centrifugal force varies. Thomson is most inclined to believe that it will not; Darwin is of opinion that it will. And among other physicists whom I have consulted a similar divergence prevails upon this point. One thinks that a lengthening of the day even by several hours will be incapable of altering the form of the solid earth; another believes that the solid earth will probably change its form just as easily as the sea. And with regard to the rapidity with which the sidereal day lengthens, opinions are just as much divided. Darwin regards as possible variations much greater than those which agree with the action of the tidal waves calculated by Thomson for recent times. It is therefore clear that this problem can hardly yet be finally solved, and that different hypotheses will be for the present admissible. We will therefore select that which is best fitted to explain the facts, assuming that the variation of the tidal wave with the eccentricity of the orbit may possibly be the cause of the periodical displacement of coast-lines. But we put forth this hypothesis with all possible reserve. Divergences of opinion between the most esteemed physicists upon this matter, and the neat manner in which the hypothesis is supported by many facts, alone give us the courage to put forward conjectures which many will probably regard as not only bold, but even improbable.

The motive force of alterations in the form of the earth should therefore be periodically variable with the eccentricity of the orbit. The sea, which is fluid, adjusts itself at once in accordance with the smallest change in the length of the day. But the solid earth offers resistance; and the day lengthens slowly and imperceptibly. With such small forces, as we have already seen, it becomes a matter of time. Even small forces can produce an effect, if they only have time to work in. It is therefore probable that the solid earth will be behind the sea in its movements. Some time will elapse before the "crust" and the inner plastic mass begin to yield. The ground under a building often begins to give way only when the building has stood for some time. If, then, the solid body of the earth lags behind the sea in its movements, and the movements both of the sea and of the solid earth occur periodically more strongly and more feebly, because the motive force is stronger and weaker according as the eccentricity of the orbit increases or diminishes, it was conceivable that the coast-lines

would come to be displaced up and down once for every time that eccentricity increases and diminishes. For there must be the greatest probability that the solid earth may yield at one place or another when the tension in the interior becomes strongest.

It is important now to examine whether the action of the tidal wave and variations in its strength are great enough to explain the displacement of coast-lines. This is a mathematico-physical problem, and it is not for me to solve it. I put it as a question for the decision of competent men, and shall confine myself to the following remarks :—

If the sidereal day has been once several times shorter, and the earth at the time was a solid body, the tension and pressure in its interior will increase with the length of the sidereal day, until finally the tension becomes so great that the earth begins to yield. It will then accommodate itself, if not in its entirety, at least partially, until the tension is equalized, at any rate in part. Perhaps then a state of repose will occur, during which a new tension will accumulate, which may introduce a new change of form. And these spasmodic changes of form in the body of the earth when strained to the limit of its power of resistance would occur precisely when the eccentricity had approached its highest value, and the tension increased most rapidly, or some time afterwards. Under such circumstances, possibly, the small variation which the tidal force undergoes with the eccentricity would turn the scale, and determine the time for the changes of the solid earth.

Thomson says (Trans. Geol. Soc. Glasgow, 1868) that it is still hopeless to attempt to solve the question of how rapidly the sidereal day lengthens, by means of tidal action. By way of trial he calculates (*l. c.* p. 26) the action of the existing tidal wave to be so great that the earth in 100 years should be retarded 180 seconds, with which corresponds a lengthening of the day of 0.01 second; and if we take this retarding power, for the sake of simplicity, as constant, the day, in 100,000 years (the time which is on the average occupied by an oscillation of the eccentricity) should become 10 seconds longer. Moreover, Thomson reckons only the marine tidal wave. To this should now be added Darwin's "interior tide," his "bodily tides," which I know no means of calculating. For many millions of years, when the moon was nearer and the tidal action considerably stronger, the day also increased more rapidly. But nowadays its increase is undoubtedly much slower, and we cannot expect great general changes of level in a short time from this cause.

To a lengthening of the day by 10 seconds (according to Todd, *l. c.*) corresponds a shortening of the equatorial radius

by 5·6 m., and a double lengthening of the polar radius, therefore, by 11·2 m. What value the lengthening of the day had in Tertiary times we do not know. It cannot well have been remarkably greater than in recent times. And it seems therefore in any case to follow, as stated above, that the vertical displacement of coast-lines can scarcely have been in general more than a few metres under any oscillation, in case our attempted explanation is correct. Therefore we must now see whether the displacement of coast-lines was so very considerable.

We must first examine how much is deposited in each precessional period, and how great is the thickness of the stages. The thickness of the deposit depends, in the first place, upon the situation of the place, whether it lies near or far from the land or the mouths of rivers, and upon the nature of the deposit; chemical deposits are commonly thinner than mechanical ones. As a mean number for each precessional period (20,000 to 21,000 years), I have obtained the following values for the different kinds of alternating deposits:—

Marl and siliceous limestone, from 0·6–2·2 m.

Clay and siliceous limestone, 1·3 m.

Marl, gypsum, siliceous limestone, marine, 1·3–1·4 m.

Ditto, freshwater, 2·8–2·9 m.

Limestone and marl, 1·8–2·5 m.

Marl, argillaceous limestone, ironstone, sandy marl, 2 m.

Sand, calcareous sandstone, marine, 2–2·3 m.

Ditto, freshwater, 3 m.

Sand, clay, ferruginous sandstone, marine, 5–6 m.

Clay, limestone, ironstone, sand, 5–7 m.

Sand, marly clay, ferruginous sandstone, lignite, up to 30–60 m.

In each stage, when there has only been one oscillation of the sea, there are usually 4 or 5 such alternating deposits; so the thickness of the stages is generally but small. I may cite the following examples. First, from the Paris basin:—the Calcaire Grossier, which represents 25 deposits and several (5–6) oscillations, is only 31·5 m. thick; Sables de Beauchamp, 13–14 m.; the Calcaire de St. Ouen, with 10 alternating deposits, is only 6–7 m.; marine gypsum, 16–17 m.; palustrine gypsum, 20 m.; Sables d'Etampes, 11–12 m.

In the Isle of Wight the beds are thicker, but also richer in mechanical deposits:—Plastic Clay, 26 m.; London Clay, 61 m.; Lower Bagshot (sand, clay, lignite, and ferruginous sandstone, with 7 alternating deposits), in all, 200 m.; Bracklesham, of the same kind as the preceding and without

any alternation, 33·5 m. ; Middle Bagshot, 91 m. ; Upper Bagshot (sand, without alternations), 37 m. ; Lower Headon, 21 m., Middle, 7 m., and Upper Headon, 26 m. ; Osborne Series, 19 m. ; Bembridge Limestone, 7·6 m. ; Bembridge Marl, 23 m. ; and Hempstead Series, 52 m.

From Belgium we have the following thicknesses :—Mon-tien (coarse limestone with Foraminifera), 93 m. ; Heersien, 32 m. ; Landenien, about 60 m. ; Yprésien, 140 m. ; Bruxel-lien, 50 m. ; Laekenien, 10 m. ; Wemmeliën, up to 80 m. (only determined by boring) ; Tongrien, 21 m. ; Rupeliën, 60 m. ; Anversien, 3–4 m. (but near Utrecht, in an Artesian well, 130 m.).

The thicknesses in the basin of Mayence are as follows :—Alzeyer Sand, 50 m. ; *Septaria*-clay, 50 m. ; Elsheimer Sands, 60 m. ; *Cyrena*-marls, 40 m. ; *Cerithium*-limestones, 25 m. ; *Corbicula*-limestones, 25 m. ; *Litorinella*-clay, 20 m. In Italy Seguenza gives the following thicknesses :—Bartonien (in part conglomerates, and perhaps several oscillations), 300 m. ; Tongrien, 50 m. ; Langhien, Astien, and Saharien, each 200 m. ; Zanclean, 300 m. The Swiss Mollasse (which is a shore-formation) is so thick that it forms whole mountains ; but, according to Charles Mayer-Eymar, the Aquitanian has a much greater and, indeed, quite exceptional thickness near Bormida, in Tuscany. Here we find (probably inclined from the first) freshwater and superiorly marine shore-formations with manifold alternations of sandstone and shales, the thick-ness of which, although it has not been exactly measured, is believed to be 3000 m., and all supposed to be formed in the Aquitanian period. And the same stage (according to Gümbel) has a similar thickness in Bavaria. Etna, which is 12,000 feet high, has been built up by volcanic eruptions in the most recent geological period, and since the Mediterranean had acquired a fauna essentially the same as at the present day.

The formation of the Mediterranean, with its strong vul-canism, has been distinguished (according to Suess and Neu-mayr) by very considerable displacements of the earth's body. The Egean Sea and the Adriatic have been formed by depres-sions in the latest geological period. Under such circum-stances, very thick deposits may be formed near land in a short time. Eocene marine deposits are uplifted 21,000 feet above the sea in folded ranges (*e. g.*, in Upper Asia). But all these are only local disturbances. If we turn, on the other hand, to localities where the conditions have been more quietly de-veloped, we find, as may be seen from the preceding statements, that the stages have only a small thickness. The deposits

which form them are partly freshwater formations, partly formations from shallow seas ; there are no well-marked deep-sea formations among them. They are to a great extent—perhaps for the most part—formed in inland seas and bays, in basins which were separated by banks from the open sea. We may arrive at this conclusion from the circumstance that salt-water and freshwater formations so frequently alternate in the Tertiary deposits ; for it is only when stratified formations take place in basin-shaped depressions that freshwater basins can be formed when the sea retires.

And if we have deep basins which are separated by banks from the open sea, a rising or sinking of the shore-line by some few metres will be sufficient to submerge or lay dry the banks. The deep basin will then alternately be salt and fresh. And a rising of the sea by a few metres will likewise suffice to cause the formation of thick saltwater deposits in the basin. If the bank then again rises a few metres, the basin will remain fresh, and thick freshwater beds can be deposited above the marine beds. In this way the formation of alternating salt- and freshwater beds may continue, under small displacements of the coast-line, until the basin is filled up.

It would seem to be more difficult to reconcile the hypothesis with the very considerable elevations which particular countries have undergone in the period which has elapsed since the Glacial period. Thus near Christiania and Trondheim the highest trace of the sea from the Postglacial time is situated 188 metres above the sea. But in other parts of our country the highest marine terraces are much lower, so that it would seem as if the elevation has not been everywhere equally great. It seems to have been weaker and weaker outwards from the centre of the country. In Southern Sweden and Denmark it has also been inconsiderable in the same period. Penck has shown (“*Schwankungen des Meerespiegels*,” in *Jahrb. Geogr. Ges. München*, Bd. vii.) that an inland ice exerts an attraction upon the sea, which, for this reason, stands higher on the coast of a country, when the land is covered with ice. The melting of the inland ice may therefore have caused the sea on our coasts to sink somewhat, but the difference between the situations of the highest marine traces in the different parts of Scandinavia is so great*, even in neighbouring localities, that it could not be explained in this way; and the most probable explanation would be that the land has risen in different degrees at different places†. It is

* See E. von Drygalski, “*Die Geoiddeformationen der Eiszeit*,” in *Zeitschr. d. Ges. f. Erdkunde in Berlin*, Bd. xxii. 1887, pp. 169 *et seqq.*

† A similar unequal elevation has probably also taken place during

also a probable supposition that the crust has not everywhere the same power of resistance to the interior pressure, and especially that the plastic mass may press in under the more yielding parts of the surface. We have a striking example of this in the laccolites noticed in North America. Eruptive matter is here pressed up from below, and has lifted the beds into dome-shaped vaults, so that the elevations have been different in degree in different places, and greatest in the middle of the domes. We may imagine that similar forces, but on a much larger scale, have contributed to the elevation of Scandinavia, —that Scandinavia is, *sit venia verbo*, as it were a laccolite on a larger scale. We must in the next place remember that the changes of the earth's surface which have taken place in the Tertiary and Quaternary periods, however great they seem to be in our eyes, are inconsiderable in relation to the whole mass of the earth. Even small forces, where they act upon a great mass, may produce very considerable local effects, provided that the changes do not everywhere occur upon the same scale. If we consider that in this way the elevations are not everywhere equally great, then a depression of the equatorial belt of only a couple of metres will suffice to cause many such countries as Scandinavia to rise many metres, and there will still remain pressure which is not exhausted.

Of course it is not said that, whenever the eccentricity has attained a high value, Scandinavia will rise to an equally great amount. If the elevation has been great in a given period, it is probable that the next period of elevation will have more difficulty in upheaving the previously elevated land. The position of the weakest points will vary. The next time, perhaps, the elevation will chiefly affect other localities. If we consider the Tertiary formations in Europe, we see that the series of deposits is nowhere complete. It is only by combining all the deposits formed at different places that we can obtain a complete outline. In part this is certainly due to the fact that the changes of form in the solid earth have not taken place simultaneously everywhere. The great eccentricities produced upheavals at different times in different places.

There is, lastly, a circumstance of great importance which may here be indicated, and which shows how quietly oscillations take place under normal conditions. Although according

earlier periods of elevation. In the Bergen conglomerate, the old shales are situated at a higher level, the further one goes from the shore. (See Kjerulf, *Udsigt over det syd. Norges Geologi*, Christiania, 1879, pp. 154–156, &c.; and Helland in *Arch. f. Math. og Naturv.* Bd. vi. Christiania, 1881, p. 222.)

to our hypothesis, the radii of the higher latitudes constantly lengthen, while those of lower latitudes are shortened, yet through long geological periods coast-lines return repeatedly, during their displacements, to their old position. Thus A. de Lapparent (*Bull. Soc. Géol. France*, sér. 3, vol. xv. p. 400) says :—"I have indicated, in the Cotentin, an agreement between the actual shores and those at which the sea stopped at various epochs of geological history. I have there shown shore-lines reproduced, almost without variations of altitude, in the Hettangian, Sinemurian, Liassian, Cenomanian, Danian, Parisian, Tongrian, Pliocene, and present epochs. . . ., and that eight or nine times at least, since the Primary era, the coincidence of the shores has been reproduced at the same point ;" and in the same work (p. 277) he says :—"It is only by tens of metres that, on the coast of the Cotentin, we must reckon the differences between the successive levels of the seas, from the Trias down to the present day." Here we see that the variations of level have taken place with great regularity. The sea has risen, and later on the land has been elevated ; and these alternate risings and sinkings have occurred with such regularity that the coast-line again and again, at long intervals, has returned about to its old place.

After this there seems really to be a possibility that our hypothesis is sufficient to explain the displacements of the shore-lines which have taken place. We have hitherto considered the conditions under high latitudes. Under lower latitudes all may sink. Here "Horste" may be formed such as Suess supposes, and as to the occurrence of these localities Lapparent's criticism is unsatisfactory. He has attacked Suess's theory of "Horste" in its entirety, but he has criticised it specially only for such localities (Colorado, Vosges, Black Forest, and the central plateau of France) as lie under high latitudes. The localities named have (according to Lapparent) risen more than their environment, which also is quite in accordance with the opinion above developed. But under lower latitudes, when a general sinking takes place in the course of time, resistant parts will form true "Horste" in Suess's sense. And it scarcely goes against our hypothesis to assume, with Suess, that the Indian Ocean is formed by depression, and that Africa, Madagascar, India, &c. are "Horste," parts of the crust which have remained in position, or which have sunk less than the neighbouring regions. In these countries, so far as their geology is known at present, there seem to be few marine formations of the Mesozoic and Cainozoic epochs.

I have said above that the different parts of the crust may

be assumed to have different powers of resistance against the interior pressure. This may, in fact, be concluded from the fact that the surface is uneven, and that old, originally horizontal formations have been upheaved unequally at different spots. In other words, there is an inequality of the surface, which has a deeper cause than the operation of eroding forces.

Changes of the earth's crust in reality happen in the most various degrees at different times. The greatest convulsions occur in the folded mountain-chains, and this has been the case in all geological periods. It is worthy of note that places where great foldings took place in ancient times seem to have been subsequently unaffected by processes of folding*. For upon the abraded summits of old folds there often lie other old formations in an undisturbed horizontal position. The most highly folded chains are also those in which plications have been continued to the latest time†. Along both sides of the Pacific Ocean from Cape Horn to the Aleutian Islands, and opposite to this along the east coast of Asia as far as the Sunda Islands, strike mighty chains associated with series of volcanoes; and from the Himalaya through the Caucasus, Balkans, Pyrenees, and Atlas a similar series of vast chains stretches through localities which are often volcanic. These highest mountains of the earth are also the youngest; they are still the least affected by the tooth of time.

But these strongly folded localities are of small extent in comparison with the other parts of the earth's surface.

On both sides of these folds there are, namely, great plateaux and plains, quite or nearly without any plications, and, on the whole, with undisturbed horizontal beds. These are Suess's "tables" (*Tafeln*). Africa, Western North America (in the Eastern there are no younger plications than from Carboniferous times), Brazil, Australia, Arabia, Persia, India, Siberia, and Russia are such "tables," in which the crust is much less disturbed. And no doubt the same thing applies to the sea-basins, or at any rate to the greater part of of them.

When the sidereal day lengthens the sea at once adjusts itself to the new conditions. It sinks under the lower and rises under the higher latitudes. According as the interior

* If the earth's axis, as some astronomers (*e.g.* Gylden) think, may shift its position in the course of time, calculations as to the pressure produced by the lengthening of the day will also change, and the situations of the parts of the crust exposed to the greatest pressure will also shift.

† The following summary is founded upon Suess's interesting studies in his great work *Antlitz der Erde*.

pressure upon the crust increases towards the poles, the opposite pressure upon the sea-bottom also increases in the same regions, because the sea rises. But the parts not covered by the sea are exposed alone to the increasing pressure from the interior without any exterior counterpressure being developed. Under lower latitudes the same thing takes place. According as the crust increases in weight the sea sinks, and the pressure upon the interior increases more rapidly in the continents, where nothing is removed, than in the sea, where the level of the water sinks. Therefore I think that the continents are weak points. The sea's movements weaken the effects of the diminishing centrifugal force for all parts covered by the sea, but the pressure acts with undiminished force everywhere on the solid land, both under low and under high latitudes. Whatever the cause may have been that originally determined the distribution of land and sea upon our globe, it seems to me that we may reasonably assume that the sea's mobility is a preservative force, which perhaps has contributed to make the continents and oceans, broadly speaking, retain their form from the most ancient times until now.

There is also reason to believe that the continents may yield more easily than the bottom of the deep sea, and that they may rise and sink more readily. And they are also separated from the depths of ocean by lines abounding in volcanoes, lines of weakness, where the connexion between the parts of the crust seems to be weaker than elsewhere. Processes of plication may also perhaps be a consequence of the movement of "tables" not being of the same kind on both sides.

But the boundaries between the deep ocean and the foot of the continents do not everywhere coincide with the existing shore. Along the coasts there are often shallow tracts in the sea. These are the foot of the land which the sea has flooded, and the great deep sea only commences further out.

[To be continued.]

L. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 363.]

March 6, 1889.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. "On the Subdivisions of the Speeton Clay." By G. W. Lampugh, Esq.

This paper gave the results of a long series of observations made

during favourable opportunities at the cliff-foot and on the beach at Speeton from 1880 to 1889. The chief points brought forward by the author were as follows:—

The sandy blue shales now seen in the cliff near Filey are not in place, but are erratics in the Drift, and most, if not all, of them are derived from the Lias.

The bituminous shales with *Belemnites Owenii*, classified as Upper Kimeridge, extend upwards to the Coprolite-bed and the beds described as Portlandian by Prof. Judd, having been wrongly placed in this part of the section. No unconformity is traceable at the Coprolite-bed, or at any other horizon, between the Jurassic and Cretaceous portions of the clays.

The clays may be most conveniently divided into zones by reference to the *Belemnites*, as follows:—

Marly shales below the Red Chalk=zone of *B. minimus* and allies.

Upper division of the “Neocomian,” including the “Cement-beds” and part of the Middle Neocomian of Judd=zone of *B. semicanaliculatus* and allies.

Lower division of the Neocomian from the top of the *Pecten-cinctus* zone down to the base of the supposed Lower Neocomian zone of *Ammonites noricus*=zone of *B. jaculum*.

From the base of the *noricus*-zone to the Coprolite-bed=zone of *B. lateralis* (zone of *Amm. Astierianus* of Judd).

The Bituminous shales below the Coprolite bed=zone of *B. Owenii* and varieties.

The clays of the zone of *Bel. lateralis* have strongly marked Jurassic affinities, and it is from this zone that the coronated *Ammonites* were obtained, these being the beds supposed by Leckenby to be of Portlandian age. A very well-marked band of nodules, with some scattered coprolitic pebbles, caps the *lateralis*-beds, and this band constituted the “Coprolite-bed” of Leckenby.

The thickness of the clays above the coprolites has been over-estimated; it is probably not more than 300 feet.

The ranges which have been assigned to some of the characteristic fossils, especially *Ammonites Astierianus*, *Amm. speetonensis*, and *Toxaster complanatus*, need to be revised and altered.

The term “Middle Neocomian,” as applied in the Speeton section, is unnecessary and misleading, seeing that a “Lower Neocomian” fauna occurs both above and below the beds with Middle Neocomian types; and, as stated by Meyer, marly shales exist between the Red Chalk and the Neocomian clays, strongly suggestive of a passage from the one to the other, and these beds contain many Gault forms. Thus there is probably at Speeton a continuous series of clays from the Jurassic to the Upper Cretaceous, and the deposition of these beds appears to have gone on contemporaneously with the erosion of the beds inland.

2. "Notes on the Geology of Madagascar." By the Rev. R. Baron. With an Appendix on some Fossils from Madagascar, by R. Bullen Newton, Esq., F.G.S.

The central highlands of Madagascar consist of gneiss and other crystalline rocks, the general strike of which is parallel with the main axis of the island, and also, roughly, with that of the crystalline rocks of the mainland. The gneiss is frequently hornblendic; its orthoclase is often pink; triclinic felspar also occurs in places; biotite is the most common mica, but muscovite is not uncommon; magnetite is generally present, often in considerable quantities. The gneiss is often decayed to great depths, forming a red soil, and the loosened rock is deeply eaten into by streams. The harder masses of gneiss, having resisted decay, stand out in blocks, and have been mistaken for travelled boulders of glacial origin. Other more or less crystalline rocks are mica-schists, chlorite-schists, crystalline limestone, quartzite (with which graphite is often associated), and clay-slate.

Bosses of intrusive granite rise through the gneiss. That east of the capital contains porphyritic crystals of felspar which near the northern edge of the granite are arranged roughly in a linear direction; here also the granite contains angular fragments of gneiss. For the most part the granite of Madagascar is clearly intrusive, but this may not always be the case.

The volcanic rocks are of much interest. The highest mountains, those lying to the S.W. of the capital, consist, in their higher parts, of a mass of lava, for the most part basaltic, but with some sandine-trachyte. The lava-streams are sometimes 25 miles long, and successive flows, up to 500 feet in thickness, are exposed by the valleys. From the great denudation which this area has undergone and from the fact that no cones now remain, we may assume that this volcanic series is of some antiquity. Of the newer volcanic series there are numerous very perfect cones, dotting the surface of the gneiss in many places. No active volcano now exists in the island, but the occasional emission of carbonic-acid gas, the occurrence of numerous hot springs and deposits of siliceous sinter, and the frequency of small earthquake-shocks, seem to show that volcanic forces are only dormant and not entirely extinct.

The ashes generally lie most thickly on the side of the cone between north and west; this is accounted for by the prevalence of the south-east trade-winds. The volcanic areas are ranged roughly in a linear direction, corresponding with the longer axis of the island.

Sedimentary rocks occur mainly on the western and southern sides of the island. The relations of these to each other have not yet been determined; but from the fossils (referred to the European standard) it seems that the following formations are represented:—Eocene, Upper Cretaceous, Neocomian, Oxfordian, Lower Oolites, Lias. Possibly some of the slaty beds may turn out to be Silurian or Cambrian. The crystalline schists, &c., are probably, for the

most part at least, Archæan. Recent deposits fringe the coasts and are largely developed on the southern part of the island.

East of the central line of watershed there is a long depression containing a wide alluvial deposit, probably an old lake-bed. Terraces fringe its sides in many places. The lagoons of the eastern coast are due to alluvial deposits.

The paper concluded with some remarks on the geological antiquity of the island, its separation dating from early Pliocene times, if not earlier. This is the conclusion arrived at by Wallace from its fauna; the author's detailed researches into its flora, recently described before the Linnean Society, show that while about five sixths of its *genera* of plants are also found elsewhere, chiefly in tropical countries, at least four fifths of its *species* are peculiar to Madagascar.

The Appendix, drawn up by Mr. R. Bullen Newton, F.G.S., consisted of Notes upon the fossils collected by the author, with tables, and descriptions of two new species, namely, *Astarte* (?) *Baroni* and *Sphæra madagascariensis*, both from deposits of Lower-Oolitic age.

3. "Notes on the Petrographical Characters of some Rocks collected in Madagascar by the Rev. R. Baron." By F. H. Hatch, Ph.D., F.G.S.

This paper was divided into two parts, the first treating of the petrographical characters of the older crystalline rocks of the eastern and mountainous part of the island, the second of the nature of the lavas that have been erupted from volcanic vents situated mainly in the same portion of the island.

i. *The Older Crystalline Rocks* are represented in Mr. Baron's collection partly by foliated specimens, partly by rocks showing no parallel structure in the hand-specimen.

The foliated specimens have, with few exceptions, the structure and composition of *gneiss*. The author subdivided them into an acid and a basic series. The acid series, which embraces rocks composed of abundant quartz with orthoclase as the dominant feldspar, he terms *granitite-gneiss*; the basic series, which consists of rocks containing little quartz and much plagioclase feldspar, *tonalite-gneiss*.

The unfoliated specimens comprise *granite*, *gabbro* or *norite*, *pyroxene-granulite*, and *pyroxenite*.

The majority of the granites are of the *granitite*-type—i. e. they are *granites with one mica*; but *granites with two micas* are also represented.

The remainder of the rocks are of a basic type. They are interesting, in the first place, on account of the striking combinations of fresh and beautiful minerals they present, as, for example :—plagioclase, hypersthene, olivine, brown hornblende and green spinel, in an *olivine-norite*; or, plagioclase, green pyroxene (omphacite or diallage), hypersthene, hornblende, garnet and iron-ore, in *pyroxene-granulite*; or, again, diallage and hypersthene in *pyroxenite*.

But of greater interest is the fact that these basic types, which

are so well known in other territories of old crystalline rocks—Saxony, Brittany, Scandinavia, Scotland, the Hudson River, etc.—constitute in Madagascar, as they do at Kilima-njaro on the adjacent mainland, a large part of the ancient platform on the submerged portions of which the sedimentary rocks have accumulated, and through which the volcanic lavas were erupted.

ii. *The Volcanic Rocks*.—In composition these are acid, intermediate and basic, mainly the latter. The acid and intermediate types described are *sanidine-trachyte* and *hornblende-augite-andesite*. The basic rocks consist of various types of *basalt*. They vary with respect to the presence or absence of corroded quartz-grains, olivine, porphyritic hornblende, and biotite. In one interesting type the hornblende appears in small idiomorphic crystals as a constituent of the ground-mass. A felspar-free variety, or *magma-basalt*, is also represented. This rock contains only a small quantity of olivine, and is therefore intermediate between Rosenbusch's *Limburgite* and Dölter's *augitite*.

March 20.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Supplementary Note to a Paper on the Rocks of the Atlantic Coast of Canada." By Sir J. W. Dawson, K.C.M.G., F.R.S., F.G.S.

In a paper in the Quarterly Journal of the Geological Society for November 1888, the author referred to the *Olenellus*-fauna as characterizing the Middle Cambrian. This fauna, he has now no doubt, from the recently published observations of Walcott and Matthew, should be regarded as characteristic of the Upper Member of the Lower Cambrian. From this arises a new view of the physical geography of the period, namely, that the Lower Cambrian was, in America, a period of continental depression, and the Middle Cambrian a period of continental elevation, leading to the important conclusion that a time of elevation intervened between the Huronian and the early Cambrian, which may represent the apparent gap between these systems in Eastern America. He thinks that this new view deserves a special mention in connexion with the probability that the Huronian and Kewenian beds are of littoral origin.

2. "The Occurrence of Colloid Silica in the Lower Chalk of Berkshire and Wiltshire." By W. Hill, Esq., F.G.S., and A. J. Jukes-Browne, Esq., F.G.S.

In the Lower Chalk of Berks and Wilts are beds which contain a large amount of disseminated colloid silica; these are comparable in general structure to the Malmstones of the Upper Greensand. Dr. Hinde's study of the latter led him to believe that the globular colloid silica which they contain was directly derived from the remains of siliceous sponges, and the authors' studies of the Chalk specimens have confirmed this conclusion by adding several important pieces of evidence.

Phil. Mag. S. 5. Vol. 27. No. 168. May 1889. 2 F

They found that the amount of free disseminated silica increases in proportion to the number of spicules and calcite-casts of spicules which occur in the rock, and observed that the great similarity between the siliceous chalk and the Malmstone was heightened by the occurrence of similar siliceous concretions in both rocks, the material of which might be described as siliceous chalk, indurated by a cement of chalcedonic silica. The conditions in which the silica was found in the Lower Chalk were described, in examples varying from those containing least to those which held most silica; in the latter the amount of colloid silica was estimated at 12·61 per cent. by weight. After noticing the vast amount of silica present in rocks with a maximum thickness of 70–80 feet, the authors discussed the difficulty of accounting for this, and drew attention to Prof. Sollas's statement that many living siliceous sponges constantly shed some of their spicules.

A further question arose as to whether the formation and accumulation of globular silica went on contemporaneously with the deposition of the calcareous material upon the sea-floor, or whether the conversion of the spicules into such silica took place after the consolidation of the rock, and the authors gave reasons for supposing that the latter was the case, the change having occurred when the rock was in a sufficiently oozy condition to admit of easy molecular distribution. Reasons were given for supposing that the disseminated colloid silica had not been derived directly from the disintegration of spicules in which a globular structure had been previously developed, but that the globular silica was precipitated from solution whilst the beds were still permeated by sea-water.

The precipitation of the chalcedonic silica was regarded by the authors as a secondary and subsequent operation. They were disposed to regard all nodular concretions resembling flints and phosphatic nodules as growths, which were more or less contemporaneous with the deposition of the materials of the enclosing rock, and in conclusion they offered some comments upon the problem of the formation of flints.

3. "Note on the Pelvis of *Ornithopsis*." By Prof. H. G. Seeley, F.R.S., F.G.S.

April 3.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "The Elvans and Volcanic Rocks of Dartmoor." By R. N. Worth, Esq., F.G.S.

The object of this paper was to give reasons for the belief that the present granite of Dartmoor passed upwards into felsitic and volcanic rocks, remnants of which are to be found in the Triassic conglomerate of Devon, in the detritus of the bottom lands of the moor itself, on the beaches of the channel, and in ancient river-gravels and pebble-beds; to indicate the wide range of character taken by the felsites of the Dartmoor district; and to point out some of the

evidence which exists in the *in-situ* elvans for the development of the most varied of these forms from a common magma.

Special opportunities for the study of two of the elvanite dykes in the neighbourhood of Tavistock have lately presented themselves. The Shillamill elvan exhibits a centre composed of quartzose felspar-porphry graduating laterally through numerous varieties into "claystone porphyry;" whilst the Grenofen elvan retains the same structure in breadth, but changes in length from a rock containing so little felsitic matter that it is essentially a fine-grained porphyritic granite to one with a compact semivitreous ground-mass, in which felspars, quartz, and mica are porphyritically developed.

As evidence afforded of the existence of distinctly volcanic rocks, mention is made of a deposit of water-borne and water-worn detritus, indicating a Dartmoor origin for a large portion of its constituents, along with rolled flints and pebbles of Carboniferous, Liassic, and Cretaceous limestone, with which were associated typical andesites and specimens of volcanic grit such as arise from the denudation of volcanic cones. This occurs on the limestone at Cattedown near Plymouth, and bears testimony to a very ancient denudation.

2. "The Basals of Eugeniocrinidæ." By F. A. Bather, Esq., B.A., F.G.S.

3. "On some Polyzoa from the Inferior Oolite of Shipton Gorge, Dorset." By E. A. Walford, Esq., F.G.S.

LI. *Intelligence and Miscellaneous Articles.*

THE SENSITIVE FLAME AS A MEANS OF RESEARCH.

BY W. LECONTE STEVENS*.

A LITTLE over thirty years ago the discovery was published in this Journal † that under certain conditions a naked flame of illuminating-gas may become sensitive to sonorous vibrations. Nine years elapsed before any development grew out of this acquisition to science. In 1867 Mr. W. F. Barrett‡, who was at that time an assistant in the laboratory of the Royal Institution, published his independent discovery of the sensitiveness of flame; and the use of the manometric flame, in the hands of Rudolph Koenig, was subsequently developed with great skill for the analysis of compound tones. The use of Professor Barrett's flame has become widely known, especially through the familiar volume of lectures on Sound by Professor Tyndall. Gavi in Italy, Barry in England, and Geyer in America independently discovered the method of securing a sensitive flame, with no pressure higher than that of the ordinary street mains, by causing air to mingle with the gas after it issues from the nozzle, and allowing the mixture to burn after passing through wire gauze. While this flame may be made exquisitely

* From an advance proof communicated by the Author.

† "On the influence of Musical Sounds upon the Flame of a Jet of Coal-gas." By J. LeConte. *Phil. Mag.* March 1858, p. 235.

‡ *Phil. Mag.* vol. xxxiii. pp. 216, 277 (1867).

sensitive, it is not so convenient in practice as the high-pressure flame of Professor Barrett. It is well known that these flames are usually sensitive only to sounds of high pitch, and through a limited range of pitch, this range becoming generally narrower with increase of sensitiveness. During the last few years Lord Rayleigh has used the sensitive flame with signal success in studying certain analogies between sound and light. His interesting lecture on "Diffraction of Sound," delivered a little over a year ago before the Royal Institution *, served as my starting-point; and I am further indebted to him for special instructions without which I should perhaps not have succeeded in performing satisfactorily all the experiments mentioned in his lecture. As this lecture has not thus far been re-published in America, a brief résumé of it may possibly be acceptable.

Waves of light are so short that special precautions are needed to exhibit the phenomena of diffraction. Light emanating from a point and interrupted by an obstacle produces a shadow that may be regarded for all practical purposes as geometric. Waves of audible sound, on the contrary, are so long that when an obstacle is interposed the effect of diffraction masks that of radial propagation, and hence it is not usually easy to make a sound shadow manifest. The difficulty in sound is not to produce diffraction, but rather to limit it by using the shortest wave-lengths possible. The pitch employed by Lord Rayleigh was more than 20,000 vibrations per second, corresponding to a wave-length of less than two thirds of an inch. To measure this the waves are reflected from a surface arranged vertically across the direction of propagation, thus producing interference with the direct waves. The position of the nodes and ventral segments is determined by moving the reflector toward or from a sensitive flame interposed between it and the source of sound. The flame flares in a ventral segment and burns quietly at a node. The distance between two points of quiescence is a half wave-length, from which the pitch is readily computed. Knowing the wave-length, if this be small in comparison with the diameter of an obstacle such as a disk, it is possible to calculate the deflexion necessary for the meeting of secondary waves behind it, from its opposite edges, in order to produce a maximum or minimum of intensity. In this way, as much as eight or nine years ago, Lord Rayleigh repeated acoustically the celebrated experiment suggested by Poisson to Fresnel, and first performed by Arago, by which a bright point was found at the middle of the shadow of a small dish. Applying the formula for Huygen's zones, an acoustic diffraction-grating was made by which sound was converged to a focus, as if by a lens, the flaring of the flame at this focus being very violent. Around it, according to the theory, there should be several successive rings of motion and quiescence, or, in other words, of noise and silence. The first ring of noise, and the rings of silence that precede and

* Proceedings of the Royal Institution of Great Britain, Jan. 20, 1888.

follow it are detected without difficulty by means of the sensitive flame.

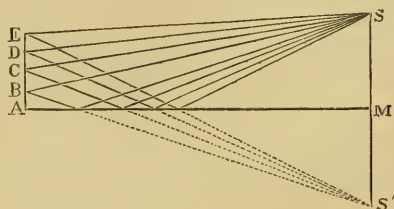
All of these experiments by Lord Rayleigh have been repeated by me. The source of sound used is Galton's adjustable whistle, through which a blast is sent from a cylinder of compressed air of oxygen. The sensitive flame is fed from a similar cylinder of compressed coal-gas, the pressure of the supply being carefully regulated in each case by means of a water manometer-gauge. The whistle is capable of giving a pitch as high as 18,000 or 20,000, but as this limit is approached the intensity becomes too much diminished, and practically the best pitch it yields is about 13,000 vibrations per second. Lord Rayleigh's whistle is slightly different in construction, and probably better than the Galton whistle. But there is no difficulty in obtaining good results with this pitch. The greatest practical difficulty is that of keeping the sensitiveness of the flame exactly right, the slightest variation of pressure making it inconstant, and causing it to give misleading indications when the attempt is made to apply it to purposes of measurement.

I have attempted by means of the whistle and flame to verify acoustically the experiment in light first performed by Grimaldi and analysed by Dr. Thomas Young, that of producing diffraction-bands by transmitting waves in the same phase through two small openings, and exploring the air with the sensitive flame for the hyperbolic lines of maximum and minimum motion. The whistle, giving forth waves 10.5 inch in length, was placed 34 inches from the screen of cardboard, whose width was two feet. Near the middle of this were cut two vertical slits, 3 inches apart, and each $\frac{1}{4}$ inch wide. The position required by theory for the hyperbolic bands was determined, the screen being at right angles to the direction of the whistle from its middle point. The middle line of maximum motion behind the screen was detected without difficulty. It was discontinuous, as might be expected when the wave-length is so considerable in comparison with the distance between the apertures. The nearest hyperbolas on the two sides of this were found in their right position, and traced back rather more than a foot from the screen, but they were not so well defined as the middle line. The next pair of hyperbolas was also found, but with poor definition. By using slits a half inch in width results were perhaps a little better, though in neither case could any measurements approximate to exactness.

Fresnel's celebrated experiment of producing interference-bands by reflexion of light from two mirrors inclined at an angle of nearly 180° was tried by Professor A. M. Mayer and myself conjointly, using sound-waves. A large plate of glass was rested on the table, and another plate inclined to it at an angle of 152° , the whistle being 67 inches from the flame, 4 inches from the inclined mirror, and 13 inches above the table. Six interference-bands were detected by means of the flame, their mean distance apart being 4 inches. By subsequent calculation this result was found correct to within a tenth of an inch. An important source of

uncertainty, however, in this experiment arises from the waves proceeding directly from whistle to flame. Even if a screen is interposed, enough space has to be left below it to allow for the passage of sound-rays reflected from the two mirrors. From the lower edge of the screen, therefore, waves are diffracted and may interfere with either or both sets of waves reflected from the mirrors. The trouble from this source caused the abandonment of this plan of experiment.

A modification of the Fresnel experiment is that of using but a single mirror, which may be rested horizontally on the table, and allowing the waves reflected from it to interfere with those radiated directly from the whistle. The effect is obviously the same as if they proceeded from two sources, but interference-bands can be produced on only one side of the median line. In the accompanying diagram AM is the plane of the mirror, S the source of sound, and S' the virtual source from which the reflected waves may be regarded as coming. Let the nozzle from which the flame issues be placed first at A and then lifted vertically. The flame will flare at the points B, C, D, &c., whose distances respectively from S and S' differ by an even number of half wave-lengths. Midway



between A and B, B and C, &c., are points of complete interference where the flame should burn quietly. The distance AB is approximately equal to $\frac{AM}{SS'}\lambda$. The accompanying table gives a comparison

between the results of theory and experiment, in which the height of the whistle above the table, MS, is 10 inches; the distance AM is 36 inches, and the wave-length, λ , is 1.05 inch. The successive measurements are of distances above the table at which the flame became quiescent. The first column is calculated from the formula; the others are the records from five sets of experiments.

Theory.	I.	II.	III.	IV.	V.
.945	.9	1.0	.9	1.0	1.0
2.835	2.8	2.7	2.7	2.8	2.9
4.725	4.7	4.6	4.7	4.9	4.7
6.615	6.7	6.7	6.7	6.9	6.7
8.505	8.9	9.0	8.8	9.0	9.1
10.395	11.0	11.3	11.0	11.2	11.3
12.285	13.6	13.6	13.6

The sensitive flame is not applicable to purposes of exact measurement, as these experiments show; but it is much more nearly so than has been generally supposed. Without its aid there would have been no possibility of establishing these important analogies between light and sound.—*American Journal of Science*, vol. xxxvii. April 1889.

ON THE LAW OF SOLUBILITY OF GASES. BY M. WOUKOLOFF.

MM. Louguinine and Khanikoff and M. Wroblewski made some researches in order to ascertain the accuracy of Dalton's law of the solution of gases, by determining the solubility of carbonic acid gas in water under pressures greater than that of the atmosphere. MM. Louguinine and Khanikoff found that carbonic acid dissolved to a greater extent than required by Dalton's law, and that the difference increased continuously with the pressure; while M. Wroblewski discovered that the absorption was always in arrear, and that the quantity of gas dissolved did not keep up with the increase in pressure. These physicists worked under conditions but little favourable to the verification of Dalton's law. This law, in its ideal form, assumes that there is no chemical action between the liquid and the gas in solution; while carbonic acid is capable of forming hydrates with water, as M. Wroblewski has shown, and the high pressures and low temperatures in the experiments of MM. Louguinine, Khanikoff, and Wroblewski must favour the formation of such hydrates.

My determinations have been made under conditions more in conformity with the requirements of Dalton's law. I give here the result of my researches on the solution of carbonic acid in carbon disulphide under feeble pressures and at various temperatures. Experiments with liquids as volatile as carbon disulphide are especially suitable for showing the general course of the phenomenon, since the limits of error are very large. Indeed, the great variation in the vapour-pressure with the temperature and, above all, the considerable alteration in this pressure caused by the gaseous atmosphere, must have a great influence on the results, although I have taken every possible precaution. I have made special experiments to determine the influence of the gas on the vapour-pressure, and to diminish the errors in the final result I have taken a considerable quantity (880 gr.) of carbon disulphide.

Let A be the quantity of gas dissolved in 1 cubic centim. of carbon disulphide (reduced to 0° and 760 millim.), P , t , the pressure and the temperature respectively, and P_1 , A_1 , the values corresponding to t_1 . Dalton's law requires

$$\frac{A}{A_1} = \frac{P}{P_1}.$$

Here are some results. In the first column are the temperatures of the experiments to be compared; in the second, the pressures

of the carbonic acid with the probable errors ; in the third, the quantities of gas dissolved, also with the errors ; and in the last, $\frac{P}{P_1}$ and $\frac{A}{A_1}$, again with the errors.

The errors are put in parentheses.

20.53	410.25 + (1.5)	0.67281 - (0.00029)	$\frac{P}{P_1} = 4.057 + \begin{pmatrix} 0.001 \\ 0.023 \end{pmatrix}$
20.59	101.11 ± (0.2)	0.16723 + (0.00002)	$\frac{A}{A_1} = 4.023 + (0.001).$
13.04	478.05 - (0.78)	0.84427 + (0.00018)	$\frac{P}{P_1} = 2.442 + (0.011).$
13.04	918.5 + (1.5)	0.33244 + (0.00023)	$\frac{A}{A_1} = 2.540 - (0.002).$
7.08	452.93 - (0.2)	0.84818 - (0.00029)	$\frac{P}{P_1} = 2.442 + (0.008).$
7.1	185.5 - (0.7)	0.33395 - (0.00013)	$\frac{A}{A_1} = 2.540 + (0.002).$

We see that if carbonic acid does not rigorously follow Dalton's law when dissolving in carbon disulphide, the deviations are very small and are of the same order as those which it shows in respect to the law of Mariotte. At low temperatures its absorption is greater, and at high temperatures less, than that indicated by Dalton's law.—*Comptes Rendus*, April 1, 1889, p. 674.

ON A VOLTAIC CURRENT OBTAINED WITH BISMUTH IN A
MAGNETIC FIELD. BY DR. G. P. GRIMALDI.

The experiments made by Nichols* on the influence of magnetism on the passivity of iron led me to investigate whether there was anything analogous in the case of bismuth.

After many fruitless attempts I made the following experiment with favourable results.

A wide U-tube contains a dilute solution of bismuth chloride in hydrochloric acid ; in the vertical limbs dip two wires of chemically pure bismuth very carefully polished.

One of these wires with the tube containing it is placed between the conical pole-pieces of a Faraday's electromagnet of medium size, in such a way that the surface of the liquid is in the most intense part of the field. The two wires are joined up to a very sensitive Thomson's galvanometer with astatic needles. On closing the circuit a current is observed in the galvanometer which might be thought to be a primary one due to a diversity of the two bismuth wires. This current, which at the outset varies rapidly, diminishes

* Silliman's Journal, vol. xxxiv. 1887, p. 419, and vol. xxxv. 1888, p. 290.

after a certain time, becoming less inconstant, and may be compensated by means of a shunt containing a standard element, and the galvanometer brought back to zero. If, then, the electromagnet is excited with a very powerful current, a *permanent deflexion is observed in the galvanometer*; if the magnetizing current is opened, especially if this be done with a certain rapidity, the galvanometer returns to zero*.

I shall publish as soon as possible the results of a special investigation of this phenomenon; for the present I restrict myself to indicating in this note the most interesting results hitherto observed.

The current produced by magnetism, and which I shall call *galvanomagnetic*, is independent of the direction and of the intensity of the primary current. Whatever be the direction of this, the galvanomagnetic current, through the bismuth fields hitherto examined, is in the galvanometer from the magnetized bismuth to the nonmagnetized one, and in the liquid from the nonmagnetic metal to the magnetic one.

While the primary current varies greatly in intensity and also in direction, the galvanomagnetic current maintains in general an almost constant intensity, and also *remains unchanged* when the primitive current passes through zero in changing its sign.

The intensity of the galvanomagnetic current depends greatly on the state of the surface of the bismuth, and to have regular results it is necessary to carefully polish the bismuth wires. To give an idea of the magnitude of the electromotive force of the galvanomagnetic current, I may mention that in the various experiments hitherto made under good conditions with various wires, and in various modifications, it has varied from $\frac{1}{12000}$ to $\frac{1}{2400}$ of a Daniell, the magnetic field being produced by a Faraday's apparatus of mean size excited by a current of 8 to 12 amperes, and with conical poles 7 millim apart.

With a less powerful magnetizing current the results are smaller; with a circuit of two amperes the galvanomagnetic current is scarcely appreciable.

The direction of the galvanomagnetic current is independent of the direction of the field; its intensity has sometimes varied a little when the field was reversed and sometimes remained constant.

This research was carried out in the Physical Institution of the Roman University with the means placed at my disposal.—*Rendiconti della R. Accademia dei Lincei*, January 6, 1889.

* It is unnecessary to say that the galvanometer was so distant from the electromagnet that it was out of the sphere of its influence. It was also ascertained that if the circuit was closed when the tube and the bismuth wires were excluded, the magnetic field did not produce any current.

WHY IRON RAILS WHICH ARE IN USE DO NOT RUST SO RAPIDLY
AS UNUSED ONES. BY W. SPRING.

The fact that iron rails on those sections of a road which are much travelled over do not rust so rapidly as in those places which are not travelled over, or, which are kept in store, has been variously explained but not in a satisfactory manner. According to the author they rust in consequence of atmospheric moisture just like other iron. Whenever a train passes over them the rust previously formed combines under the joint influence of the pressure and friction to form magnetic oxide of iron; according to the author this protects the iron in consequence of the electrical polarity which it imparts by making it passive, and thereby preserves the rail from further destruction. In confirmation of this explanation the author pressed ferric hydrate between iron plates under a pressure of 1000 to 1200 atmospheres. If the ferric hydrate was dry there was no reaction; but if the hydrate was moistened with a few drops of water, then, near the iron, it became black and adhered to the metal; the surface of the plates was visibly attacked, and quantitative analysis showed the presence of magnetic oxide. The author holds that this is the process which naturally takes place. On the surface of the rails, where the wheels pass, he has found magnetic oxide mixed with varying quantities of ferric oxide, and a small quantity of pure iron.—*Bull. de l'Acad. Roy. de Belge*, xvi., p. 47, 1888; *Beiblätter der Physik*, vol. xiii. p. 122.

EXPERIMENTAL RESEARCHES ON THE TENSION OF THE VAPOUR
OF SOLUTIONS. BY F. M. RAOULT.

The object of the present research is to investigate how far the tension of volatile liquids varies when different solids are dissolved in them. The quantities to be determined are the tensions of the vapour f of a pure volatile solvent, and then f' , that of the same solvent containing in solution a known weight of the solid, the temperature remaining constant.

Two methods were used—a statical and a dynamical one. The statical is essentially that of Dalton, and consists in measuring the depression produced in barometric columns, when equal volumes of the pure solvent and of the solution are severally introduced. This method, the details of which are described, gives the most accurate results.

The dynamic method is easily applied when the law is known according to which the vapour-tension of the pure solvent varies with the temperature. It is sufficient then to boil successively the solvent and the solution in the same reflux apparatus, and to note exactly their boiling-points under the atmospheric pressure. In these conditions the vapour-tension of the boiling solution is given

A distinct and proper deflexion was now observed.

A wire was then taken 10 times round the ring, and connected to a common quadrant electrometer. The deflexion was easy to see. It could also be just seen with only one turn of wire.

This kind of experiment seems likely to furnish a method of determining " v "; and it ought to be a good method, because it is v^2 which is really measured.

However, what I had wanted to see was not any effect assisted by the concentration of a conductor, but a result in free dielectric. And this at length has been obtained.

All copper screens and everything of the sort were removed. The ring was wound with only a few turns of thick wire so as to reduce the slope of potential needed to propel a current through it. The wire was wound symmetrically in two halves, and the far ends were connected to a plug-key, so that one might work with either closed or open circuit.

The reversing-key was a quite distinct mercury arrangement specially made for strong currents, fixed to a table on which stood the screen for receiving the spot of light, which was a small disk of translucent paper in a drum of tin plate mounted on an independent stand. The light of a lamp was brought on to the mirror independently of this stand by a 45° looking-glass. Such a screen is handy, because one can follow the rather erratic spot of light all over the table, wherever it chooses to take up its temporary abode.

The middle of the battery was put to earth, so that the potentials of the two halves of the wire might be equal and opposite. The needle was then adjusted by trial and error, its glass box being pushed in and out of the ring, its suspending fibre being turned by a tangent screw, &c., until the electrostatic effect obtained by working the reverser on open circuit was very small. It could never be got quite zero; and even when small it would not always remain small for long.

However, on now closing the circuit by the plug-key, the residual electrostatic effect would be certainly largely reduced, and the wished-for effect might be seen. It was seen; the hair-line of the spot of light oscillated over two or three millimetres when the key was worked in unison with the vibrating arm. It assisted the residue of the electrostatic effect when the charging of the needle was in one direction, and it neutralized or reversed the residual electrostatic effect when the charging of the needle was inverted.

It is this reversal of whatever disturbing effect remained

which I depend upon as a proof of the correctness of the observation.

Added later (May 10).

More recently Davies has noticed a peculiar disturbance when working on open circuit, and when the residual electrostatic effect is very small; viz., that it is not constant in direction. It begins with a kick one way, and then a larger motion the other way. By working the reversing-key quickly one kind of swing can be got up; by working it slowly, the other kind of swing can be got up.

I do not know the meaning of this effect. It is difficult to believe that the fact that the wire is coiled round iron causes the electrostatic charging of its ends to be slow; though that is the kind of thing it looks like. More probably it is due to some trivial disturbing cause, but the effect is singularly clear and persistent.

To get over the electrostatic effect more completely, and still without the use of a screen, a single layer of copper ribbon was wound on, in about 10 turns, and its outer turn was put to earth.

Another disturbing effect was now noticed: it reversed with the current, and it reversed also when the needle was turned through 180° , but it did not reverse when the charge was simply reversed; nor did it cease when the needle was uncharged. Moreover it was not altogether temporary; there was a residue permanent. Plainly it was a direct magnetic effect.

On taking a map of the field with iron filings, a considerable number of lines of force were found to be leaking across the ring, and these would be able to deflect the needle if it had a trace of permanent magnetization; which it is quite likely to acquire.

It was taken out and tested between the poles of a powerful magnet—the gelatine was distinctly magnetic, the shellac was feebly diamagnetic.

A number of substances were now tried in order to find something sensibly devoid of magnetic properties, and ultimately a paper was found which was barely diamagnetic. With this a new needle was constructed. Wire was once more wound all over the ring, and its field tested with iron filings till hardly any stray lines could be found.

The mirror was removed, and a small pointer supplied instead, so as to be read with a microscope. And in this way the last observations have been made.

by the barometer, that of the pure solvent by a table prepared in advance.

It follows, from the researches of Von Babo, Wüllner, and others, that the ratio $\frac{f-f'}{f}$ between the vapour-tension of a salt in aqueous solution and the tension f of pure water is sensibly independent of the temperature; the exceptions to this rule are probably due to an incipient dissociation of the salts dissolved.

The author has investigated whether this holds also for other solvents, and has found that it does with sufficient nearness.

The experiments of Von Babo and of Wüllner have shown that if a number N of molecules of a salt be dissolved in 100 grammes of water, the vapour-tension undergoes a diminution which, for the same temperature, is proportional to the number of molecules dissolved. This is expressed by the formula

$$\frac{f-f'}{fN} = K,$$

where K is a constant depending on the nature of the salt.

This law only holds if the solutions are very dilute; for stronger solutions the formula should be replaced by the following one:

$$\frac{f-f'}{fN'} = K;$$

where N' is the number of molecules of the fixed substance contained in 100 molecules of the *mixture*. The value of N' is given by the formula

$$N' = \frac{100N}{100+N}.$$

The ratio $\frac{f-f'}{fN'}$ being what is called the *relative diminution of vapour-tension* of the substance in question, the latter formula may be thus expressed in ordinary language. *For all solutions of the same kind the relative diminution of vapour-tension is proportional to the number of molecules of the solid dissolved in 100 molecules of the mixture.*

The author cites a series of experiments made with benzoate of ethyl, dissolved in ether in proportions varying from 9 to 97 per cent., the results of which closely agree with those calculated by the modified formula.

The author further found that there is a relation between the lowering of the freezing-point and the diminution of the vapour-tension of the same solution. *For dilute aqueous solutions of the same nature and the same concentration, the number which expresses the relative tension of the vapour is always near the $\frac{1}{100}$ part of that which expresses the lowering of the freezing-point.*

This holds also for other solvents than water; thus, with benzine the ratio appears to be about 63.

It thus appears from these researches that :—

1. Any solid which dissolves in a volatile liquid diminishes the vapour-tension of this liquid.

2. In all volatile solvents the molecular diminution of the vapour-tension due to different compounds in solution approaches two mean values, which vary with the nature of the solvent, and one of which, called *the normal*, is twice that of the other: it is that which is most frequently produced.

As determined from a large number of solvents, values are given for this constant which amount to 0·0104.

Hence it may be said that even 1 molecule of a solid dissolved in 100 molecules of any volatile solvent diminish the vapour-tension of the liquid by an almost constant fraction of its value, and near to 0·0104.

The formula

$$\frac{f-f'}{fN} = 0\cdot0104$$

may be utilized to determine the molecular weight of solid or volatile bodies. If P is the weight of a substance dissolved in 100 grammes of a volatile liquid, M' the molecular weight of the solvent, and M that of the body dissolved, we have

$$N = \frac{PM'}{M}, \quad \text{from which} \quad \frac{M}{M'} = 0\cdot0104 \times \frac{fP}{f-f'}.$$

It is thus possible to calculate M when M' is known, and conversely.

This method of determining molecular weights is more difficult to carry out, and is less exact than the *cryoscopic method*, which depends on the freezing-point of solutions; but it may be of great service in many cases.—*Journal de Physique*, [2] vol. viii. p. 1.

ON CHEMICAL ACTION BETWEEN BODIES IN THE SOLID STATE.

BY W. SPRING.

The author mixed copper filings with perfectly dry pulverulent mercuric chloride, and kept the mixture in closed glass tubes which he shook from time to time. A very slow decomposition of the two bodies set in, with the formation of cuprous and mercurous chlorides. In like manner there was a decomposition of dry potassic nitrate with powdered sodic acetate freed from water of crystallization; for, after standing for four months in the drying-vessel, the mass was deliquescent in the air, from which the presence of potassic acetate may be inferred, since the original salts are not deliquescent. This reaction takes place much more rapidly at a high temperature; for although the melting-point of the two salts is above 300°, the mixture in question fused in a water-bath in three hours to a white mass, which was also seen to be deliquescent in air.—*Bull. de l'Acad. Roy. de Belge*, vol. xvi. p. 43, 1888; *Beiblätter der Physik*, vol. xiii. p. 123.

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LII. *On the Electromagnetic Effect of Convection-Currents.*
By Prof. HENRY A. ROWLAND, and CARY T. HUTCHINSON,
Fellow in Physics, Johns Hopkins University *.

[Plate IX.]

THE first to mention the probable existence of an effect of this kind was Faraday †, who says :—" If a ball be electrified positively in the middle of a room and then be moved in any direction, effects will be produced as if a current in the same direction had existed." He was led to this conclusion by reasoning from the lines of force.

Maxwell, writing presumably in 1872 or 1873, outlines an experiment, similar to the one now used, for the proof of this effect.

The possibility of the magnetic action of convection-currents occurred to Professor Rowland in 1868, and is recorded in a note-book of that date.

In his first experiments, made in Berlin in 1876, Prof. Rowland used a horizontal hard rubber disk, coated on both sides with gold, and revolving between two glass condenser-plates. Each coating of the disk formed a condenser with the side of the glass nearer it ; the two sides of the disk were charged to the same potential. The needle was placed perpendicular to a radius, above the upper condenser-plate, and nearly over the edge of the disk. The diameter of the hard rubber disk was 21 centim., and the speed 61 per second.

* Communicated by the Authors.

† Experimental Researches, vol. i. art. 1644.

The needle system was entirely protected from electrostatic effect. On reversing the electrification deflexions of from 5 to 7.5 millim. were obtained, after all precautions had been taken to guard against possible errors. Measurements were made, and the deflexions as calculated and observed agreed quite well; but it was not possible to make the measurements with as great accuracy as was desired, and hence the present experiment.

Helmholtz*, in 1875 and later, carried out some experiments bearing on this subject. According to the "potential theory" of electrodynamics which he wished to test, unclosed circuits existed. The end of one of these open circuits would exert an action on a close magnetic or electric circuit. So the following experiment was made by M. Schiller†, under his direction.

A closed steel ring was uniformly magnetized, the magnetic axis coinciding with the mean circle of the ring. This was hung by a long fibre and placed in a closed metal case. A point attached to a Holtz machine was fixed near the box, and a brush-discharge was kept up from this point. If the point acted as a current-end, a deflexion would be expected, on the potential theory. No deflexion was observed, although the calculated deflexion was 23 scale-divisions. The inference is that either the potential theory is untrue, or else that there is no unclosed circuit in this case, *i. e.* that the convection-currents completing the circuit have an electromagnetic effect.

Schiller's further work, not bearing directly upon convection-currents, leads him to the conclusion that all circuits are closed, and that displacement-currents have an electromagnetic effect.

Dr. Lecher is reported to have repeated Professor Rowland's experiment, with negative results. His paper has not been found.

Röntgen‡ has discovered a similar action; he rotates a dielectric disk between the enlarged plates of a horizontal condenser and gets a deflexion of his needle. He apparently guards against the possibility of this being due to a charge on his disk. A calculation of the force he measures shows it to be almost one eighth of that in the Berlin experiment. His apparatus is not symmetrically arranged, the disk being much closer to the upper condenser-plate; the distances from the upper and lower plates are 0.14 and 0.25 centim. respectively.

* *Wiss. Abh.* i. p. 778.

† *Pogg. Ann.* clix. p. 456.

‡ *Sitzb. d. Berl. Akad.* Jan. 19, 1888.

He uses a difference of potential corresponding to a spark-length of 0.3 centim. in air between balls of 2 centim. diameter, *i. e.* about 33 electrostatic units, equal to the sparking potential between plane surfaces at 0.26 centim. The disk is an imperfect conductor, and altogether it does not seem clear, in spite of the precautions taken, that this is not due to convection-currents.

In the Berlin apparatus, as stated above, the needle is near the edge of the disk; the magnetic effect produced is assumed to be proportional to the surface-density multiplied by the linear velocity; hence the force will be much greater at the edge of the disk than near the centre; but the field will be more irregular, and so make accurate measurements more difficult.

In the present apparatus a uniform field is secured by using two vertical disks rotating about horizontal axes in the same line; the needle system is placed between the disks, opposite their centres. The disks are in the meridian; they are gilded on the faces turned towards the needle. Between the disks are placed two glass condenser-plates gilded on the surfaces near the disk; and between these glasses is the needle. The whole apparatus is symmetrical about the lower needle of the astatic system.

Each disk is surrounded by a gilded hard rubber guard-plate in order to keep the density of the charge uniform at the edges. The guard-plates are provided with adjusting-screws to enable them to be put accurately in the plane of the disks; and the glass plates in turn have adjusting-screws for securing parallelism with the guard-plates. The glass was carefully chosen as being nearly plane. Disks, glass plates, and guard-plates all have radial scratches, to prevent conduction-currents from circulating around the coatings.

In the periphery of the disk are set eight brass studs which penetrate radially for about 5 centim., then turning off at a right angle run parallel to the axis until they come out on the surface of the disks. They there make contact with the gold foil. Metal brushes set in the guard-plate bear on these studs, and in this way the disks are electrified.

The figure (Pl. IX. fig. 1) gives a vertical projection of the entire disk-apparatus:—D D are the disks; G G G G the guard-rings; Y Y Y Y the condenser-plates; R R R R hard rubber rings fitting on the shoulders A A; X X X X bearing-boxes for the axle; P P P P supporting-standards; E E metal bases sliding in the bed B B, and held in any position by screws Z; F F the bases carrying the glass plates, sliding in the same way as the others. S S S S are the adjusting-screws

for the guard-plates, and tt for the glass plates. $LLLL$ are collars for catching the oil from the bearings; $CC, C'C'$ are speed-counters, CC gear with the axle, and $C'C'$ with CC in the manner shown; each has 200 teeth, and speed-reading is taken every 40,000 revolutions.

The needle system is enclosed in the brass tube T , ending in the larger cylindrical box in which are the mirror and upper needle. This is closed in by the conical mouth-piece Q , across the opening of which is placed a wire grating. The mirror is shown at M , the upper needle at N' and the lower at N . The system is hung by a fibre-suspension about 30 centim. in length, protected by a glass tube. The needle-system is made by fitting two small square blocks of wood on an aluminium wire; on two sides of each of the wooden blocks are cemented small scraps of highly magnetized watch-spring. The needle thus made is about $1 \times 1 \times 10$ millim.

The mirror is fixed just below the upper needle, and is read by a telescope 200 centim. distant. The plane of the mirror is at an angle of 45° with the plane of the disks for convenience. The whole is supported by the board OO attached to a wall-bracket.

Two controlling magnets (WW) with their poles turned in opposite directions are used. By means of the up and down motion of either magnet, any change in the sensitiveness can be attained; and by the motion in azimuth, the zero point is controlled. The advantage of its use lies in the extremely delicate means it affords of changing the sensitiveness, much more delicate than with a single magnet.

The bed-plate B is screwed to one end of a table, at the other end of which a countershaft is placed (fig. 2). This is run by a electric motor in the next room, the belt running through the open doorway. The motor is 14 metres from the needle.

Although the disks and countershaft were carefully balanced when first set up, and the table braced and weighted by a heavy stone slab, yet at the speed used, 125 per second, the shaking of the entire apparatus was considerable; the needle was so unsteady that it could not be read. This was seen to be due to vibrations of the telescope itself and not to the needle. To prevent it, each leg of the table on which the telescope rested was set in a box about 30 centim. deep filled with sawdust, and a heavy stone slab was placed on top of this table. This entirely did away with the trouble; the swing of the needle was as regular when the apparatus was revolving as when it was at rest.

The two hard rubber rings (RR) mentioned above have

grooves cut in their peripheries ; in these grooves wires are wound. These serve as a galvanometer for determining the needle-constant. When not in use they are held in the position shown in the figure, but when it is desired to determine the needle-constant they are slipped on the shoulders (AAAA) and pushed up in contact with the back of the disks. Each has two turns : this arrangement will be referred to as the disk-galvanometer.

If a known current is sent through the disk-galvanometer, and the geometrical constant be known, the part of the constant depending on the field and needle is determined.

The current is measured by a sine-galvanometer, placed in another part of the room. To determine H at the sine-galvanometer a metre brass circle is put around the sine-galvanometer, and the needle of the latter used as the needle of the tangent-galvanometer thus made. Using this tangent-galvanometer in connexion with a Weber electro-dynamometer, H at the sine-galvanometer is measured.

The charging was by a Holtz machine connected to a battery of six gallon Leyden jars. These latter are in circuit with a reversing-key, an electrostatic gauge, and the disks.

The potential was measured by a large absolute electrometer ; all previous observers have used spark-length between balls, with Thomson's formula. Greater accuracy is claimed for this work, largely on this account.

In this instrument the movable plate is at one end of a balance-arm, from the other end of which hangs, on knife-edges, a balance-pan. This movable plate is surrounded by a guard-ring.

The lower plate is fixed by an insulating rod to a metal stem, which slides up and down in guides. The distances are read off on a scale on the metal stem. The zero reading is got by inserting a piece of plane parallel glass whose thickness has been measured. The lower plate and guard-ring have a diameter of 35 centim., and the movable disk a diameter of 10 centim.

The routine of the observations was as follows:—A determination of H and the needle-constant (β) was first made. The electrostatic gauge was then set at a certain point, and readings of difference of potential were taken. The disks were now started, electrified, and a series of three elongations of the needle taken ; the electrification reversed and three more elongations taken, &c.

About every five minutes speed-readings had to be noted, and at each reversal it was necessary to replenish the charge in order to keep the gauge-arm just at the mark. In this way

a "series" of readings consisting of about 25 reversals was made. After the series, electrometer readings were again taken; the conditions were then changed in some way, and another series begun.

The circumstances to be changed are :—distance of disks from needle ; distance of glass plates from needle ; electrification ; and direction of rotation.

The calculation of the deflexion is based on the assumption that the magnetic effect of a rotating charge is proportional to the quantity of electricity passing any point per second, just as with a conduction-current. Below are the formulæ used.

In the equations the letters have the following meanings. All quantities are given in terms of C.G.S. units.

X = Distance from centre of disk to lower needle.

r = " " " upper needle.

c = Radius of disk.

l = Distance between needles.

a = Radius of windings of disk-galvanometer.

b = Distance, centre of disk-galvanometer to lower needle.

ρ = Distance, centre of disk-galvanometer to upper needle.

N = Number of revolutions per second.

σ = Surface-density of electrification in electrostatic measure.

V = Ratio of the units.

α = Angle of torsion of the electro-dynamometer.

ϕ = Angle of deflexion of sine-galvanometer.

δ = Angle of deflexion of tangent-galvanometer.

Δ = Change of zero-point on electrifying the disks = half the charge on reversing.

s = Scale-reading for disk-galvanometer.

w = Weight on pan of electrometer.

D = Electrometer reading.

e = Distance of glass plates and disks

= Condenser distance.

Force, in the direction of the axis, due to a circular current of radius c , at a distance x on the axis

$$= 2\pi I \frac{c^2}{(c^2 + x^2)^{\frac{3}{2}}}.$$

Strength of convection-current

$$= 2\pi cdc \frac{N\sigma}{V}.$$

∴ total force due to the disk of radius c

$$= 4\pi^2 \frac{N\sigma}{V} \int_0^c \frac{c^3 dc}{(c^2 + x^2)^{\frac{3}{2}}}.$$

$$= 4\pi^2 \frac{N\sigma}{V} \left\{ \frac{c^2 + 2x^2}{(c^2 + x^2)^{\frac{1}{2}}} - 2x \right\} = 4\pi^2 \frac{N\sigma}{V} \cdot A;$$

and for the two disks acting in the same direction, total force

$$X = 8\pi^2 \frac{N\sigma}{V} \cdot A.$$

This gives the force on the lower needle.

Correction for the upper needle :—

Potential at any point due to a circular current,

$$V' = \int I d\varpi;$$

ϖ equals the solid angle subtended at the point by the circle

$$= 2\pi \left\{ \frac{1}{2} P_1 \left(\frac{c^2}{r} \right) - \frac{1 \cdot 3}{2 \cdot 4} P_3 \left(\frac{c^2}{r} \right)^4 \dots \right. \\ \left. - (-)^i \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i} P_{2i-1} \left(\frac{c}{r} \right)^{2i} \right\}$$

($i=1 \cdot 2 \cdot 3 \dots$)

$$\therefore V' = 2\pi I \Sigma_i \left\{ -(-)^i \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i} P_{2i-1} \left(\frac{c}{r} \right)^{2i} \right\}.$$

Substituting the value of I , we have as the potential of the disk

$$2\pi \Sigma_i \int_0^c 2\pi c \frac{N\sigma}{V} \left\{ -(-)^i \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i} P_{2i-1} \left(\frac{c}{r} \right)^{2i} \right\} dc,$$

$$V = 4\pi^2 \frac{N\sigma}{V} c^2 \Sigma_i \left\{ -(-)^i \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i(2i+2)} P_{2i-1} \left(\frac{c}{r} \right)^{2i} \right\}.$$

But

$$P_i = (-)^i \frac{r^{i+1}}{1 \cdot 2 \dots i} \frac{\partial^i}{\partial x^i} \left(\frac{1}{r} \right).$$

and

$$\frac{\partial}{\partial x} P_i = \frac{i+1}{r} \left\{ \mu P_i - P_{i+1} \right\} - \mu = \frac{x}{r}.$$

∴ The force

$$\begin{aligned}
 &= -\frac{\partial V}{\partial x} \\
 &= 4\pi^2 \frac{N\sigma}{V} c^2 \sum_i \left[(-)^i \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i \cdot (2i+2)} \left(P_{2i-1} \left\{ -\frac{2ic^{2i} \cdot x}{r^{2i+1}} \right\} \right. \right. \\
 &\quad \left. \left. + \left(\frac{c}{r} \right)^{2i} \left\{ \frac{2i}{r} (\mu P_{2i-1} - P_{2i}) \right\} \right) \right] ; \\
 &= 4\pi^2 \frac{N\sigma}{V} c^2 \sum_i \left\{ (-)^{i+1} \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2(i+1)} \frac{2i}{r} \left(\frac{c}{r} \right)^{2i} P_{2i} \right\} ;
 \end{aligned}$$

and for the two,

$$X_1 = 8\pi^2 \frac{N\sigma}{V} \frac{c^2}{r} \left[\frac{1}{4} P_2 \left(\frac{c}{r} \right)^2 - \frac{1 \cdot 3}{2 \cdot 6} P_4 \left(\frac{c}{r} \right)^4 + \dots \right],$$

where the sign of the entire expression has been changed, since the poles of the upper and lower needles are opposite.

Or

$$X_1 = 8\pi^2 \frac{N\sigma}{V} \cdot B.$$

Needle constant:—

For the disk-galvanometer windings have in the same way, for the lower needle, force due to current I in one turn

$$\begin{aligned}
 &= 2\pi I \frac{a^2}{(a^2 + b^2)^{\frac{3}{2}}} \\
 &= 2\pi I \cdot C.
 \end{aligned}$$

For the four turns,

$$X' = 8\pi I \cdot C.$$

Upper needle.—The force is got in the same way as for the disk, omitting the integration, *i. e.* we must multiply the general term of B by $\frac{2(i+1)}{a^2}$ and replace $2\pi \frac{N\sigma}{V}$ by I . This gives

$$2\pi I \sum_i \left\{ (-)^{i+1} \frac{1 \cdot 3 \dots (2i-1)}{2 \cdot 4 \dots 2i} \frac{2i}{\rho} \left(\frac{a}{\rho} \right)^{2i} P_{2i} \right\} ;$$

a replacing c , and ρ , r .

For the total force,

$$X_1' = \frac{8\pi I}{\rho} \left[P_2 \left(\frac{a}{\rho} \right)^2 - \frac{3}{2} P_4 \left(\frac{a}{\rho} \right)^4 + \dots \right],$$

or

$$X_1' = 8\pi I \cdot D.$$

Forces acting on the needle system :—

Let M = moment of lower needle,

M' = „ upper „

then

Couple on lower needle due to field = $HM \sin \theta$,
 „ upper „ „ = $-H'M' \sin \theta$.

Total couple = $(HM - H'M') \sin \theta$.

Due to disk-galvanometer :

Couple on lower needle = $MX' \cos \theta$,
 „ upper „ = $M'X_1' \cos \theta$.

Total couple = $\{MX' + M'X_1'\} \cos \theta$,
 = $8\pi I \{MC + M'D\} \cos \theta$.

\therefore for equilibrium,

$$8\pi I \{MC + M'D\} \cos \theta = \{HM - H'M'\} \sin \theta,$$

or

$$I = \frac{(HM - H'M') \tan \theta}{8\pi(C + D)M \left\{ 1 + \frac{D}{C} \left(\frac{M'}{M} - 1 \right) \right\}}.$$

But $\frac{D}{C} = 0.03$ nearly, and $\frac{M'}{M}$ is approximately unity.

$$\therefore I = \frac{(HM - H'M')}{8\pi M(C + D)} \tan \theta,$$

or

$$\frac{(HM - H'M')}{M} = \frac{8\pi(C + D)}{\tan \theta} I = \beta \text{ (say).}$$

Similarly, for the revolving disks,

$$8\pi^2 \frac{N\sigma}{V} \{A + B\} = \frac{(HM - H'M')}{M} \tan \Delta.$$

$$= \beta \tan \Delta.$$

$$\therefore V = 8\pi^2 \frac{N\sigma}{V} \frac{A + B}{\beta \cdot \Delta}.$$

For the sine-galvanometer :

$$I = \frac{H}{L} \sin \phi.$$

$$\Gamma = 1831.$$

$$\therefore I = 10^{-4} 5.46 H \sin \phi,$$

and

$$\beta = 10^{-4} 5.46 H \frac{8\pi(C+D)}{\tan \theta} \sin \phi.$$

For measurement of H:—
Electrodynamometer,

$$i = g \frac{\sqrt{K}}{T} \sqrt{\sin \alpha}.$$

g = constant of windings = $10^{-3} 6.454$
 K = moment of inertia = $10^2 8.266$.
 T = time of one swing = 2.441 .
 $\therefore i = 10^{-2} 7.59 \sqrt{\sin \alpha}.$

Tangent galvanometer:—

$$i = \frac{H}{G} \tan \delta = \frac{H \cdot b}{2\pi n} \tan \delta.$$

n = no. turns = 10.
 b = radius turns = 49.98 .
 $\therefore i = 0.795 H \tan \delta,$

and, substituting the value of i ,

$$H = 10^{-2} 9.55 \frac{\sqrt{\sin \alpha}}{\tan \delta}.$$

Surface density (σ):—

σ is obtained from electrometer-readings.

$$\sigma = \frac{V}{4\pi e}.$$

$$V = D \sqrt{\frac{8\pi g w}{A}}.$$

A = corrected area of movable plate
 $= \frac{1}{2}\pi \{R^2 + R_1^2 + \dots\} = \frac{1}{2}\pi \{51.01\}.$

$$\therefore V = 10 \times 1.756 D \sqrt{w},$$

and

$$\sigma = 1.397 \frac{D}{e} \sqrt{w}.$$

As soon as the attempt was made to electrify the apparatus, difficulties of insulation were met with. The charged system was quite extensive, and the opportunity for leakage

was abundant ; in addition, the winter here has been very damp. Most of the trouble of this kind has been due to the glass in the apparatus ; in no case where glass was used as an insulator has it proved satisfactory, not even when the air was dry. First, the stand with glass legs, on which the Leyden-jar battery was placed, was found to furnish an excellent earth-connexion.

Paraffin blocks interposed stopped this. The reversing-key had three glass rods in it, all of which were found to leak ; six different specimens of glass, some bought particularly for this as insulating glass, were all found to allow great leakage. Shellacing had no effect. Hard rubber was finally substituted for glass ; and after that the key insulated very well, even in damp weather.

On charging the glass plates, the disks being earthed, it seemed almost as if there was a direct earth-connexion, so rapid was the fall of the charge. This was not regarded at the time, as the plates were always kept earthed ; but later, when it became necessary to charge the plates, the insulation had to be made good.

Investigation showed that this was caused by leakage directly through the substance of the glass to the brass back-pieces (H H). Hard rubber pieces were substituted, and the trouble was entirely removed.

There was at first a deflexion in reversing the electrification while the disks were at rest. This was of course due to direct electrostatic effect ; but it was not for some time clear where the point of weakness in the electrostatic screen lay. It was found to be the faulty contact between the tinfoil covering of the glass tube and the brass collar ; the brass had been lacquered. After this was corrected there was never again any deflexion on reversing the charge, although the precaution was taken of testing it every day or so.

The currents induced in the axle by the rotation caused no inconvenience ; if the disks are rotated in the same direction their effect is added, while the effect of the axles is in opposite directions. Even when the disks were rotated oppositely, the deflexion due to the axles was only 3 or 4 cm., and remained perfectly constant.

On running the disks, unelectrified, without the glass plates between them and the needle, a deflexion of 4 or 5 cm. was noticed. This was perfectly steady deflexion, and could easily be shown to be due to the presence of the plate, as it ceased when the plates were replaced.

This was very troublesome for a time, especially as the presence of a brass plate in place of the glass was found to

diminish the deflexion, but did not bring the needle back to zero as the glasses did. On looking at the figure (Plate IX. fig. 1) it will be seen that there is a brass plug (I) closing the bottom of the tube in which the needle is placed. The rapid rotation of the disks caused a very appreciable exhaustion at the centre, and consequently a steady stream of air was sucked down the tube through the open mouthpiece, and out through the imperfect connexion of the plug. Air-currents were not at first suspected, as the deflexion was so very steady. The brass plate used was smaller than the glass, and hence did not completely shield the tube.

After the brass back-pieces (H H) had been taken out, and a hard rubber substituted, it was found that with one direction of rotation the needle was extremely unsteady; it would run up the scale for several centimetres, stop suddenly, &c.—evidently a forced vibration. This was traced to air-currents also. Now, the air blew into the open mouth of the cone. The apparatus had been run for some months with this open, and not the slightest irregularity had been seen. But the hard rubber pieces were very much larger than the brass ones which were removed; they filled up the lower space to a greater extent, and deflected the air upwards more than before, causing the unsteadiness. With the opposite rotation the air was thrown down instead of up, and consequently did not affect the needle.

The first systematic observations were made in January 1889, with the disks charged and plates earthed. The deflexion on reversing was got without difficulty, and it was in the direction to be expected; that is, with positive electrification, the effect was equivalent to a current in the direction of motion of the disk. A number of series were taken in the next two months; they agreed among themselves well enough, but did not follow the law assumed. The deviation can best be explained in this way:—The equations above show that for a fixed position of the disks $\Delta \propto \sigma \frac{N}{\beta} \frac{D}{e} \propto \frac{N}{\beta}$. If then, N and β being constant, the condenser plates are moved up to the disk, step by step, thus varying e , and D be changed at the same time so as to keep $D/e \propto \sigma$, a constant, the deflexions should be constant.

Such was not found to be the case; the deflexions were directly proportioned to e instead of being constant: that is, with greater difference of potential, the deflexions were greater, although the surface-density remained constant. Finally this was found to be due to a charge on the back surface of the gold coating. The end of the axle comes

nearly up to the surface of the disk and taken with all the brass work must form a condenser of a certain capacity with the inner face of the gold foil.

This made a change necessary in the method of working; the disks had to be earthed and the glasses charged. This was done; but now the deflexions were found always to be greater with positive rotation (Zenith, North, Nadir, South) than with negative.

It was considered possible that the brushes might have something to do with this, so they were taken off. Earth connexion with the disk was made by drilling through to the surface of the disk in the line of the axle and setting in a screw, which came flush with the surface and also made contact with the axle; this, however, made no difference, the deflexions for negative rotation were always smaller.

Table I. gives the results of a number of observations. All were taken with the plates charged and the disks earthed by means of the axle.

The meaning of the letters has been given; $1/\beta$ is directly proportioned to the needle sensitiveness.

TABLE I.

No.	Rotation.	x .	e .	N.	σ .	$1/\beta$.	2A.	V.
							mm.	
1	+	2.54	1.24	122	1.16	$1.50.10^5$	5.3	$2.42.10^{10}$
2	+	2.57	"	125	1.30	3.11	9.0	3.38
3	+	"	"	129	1.23	2.15	6.94	3.00
4	-	"	"	129	1.23	"	5.58	3.68
5	+	"	1.21	127	1.21	2.25	5.6	3.74
6	-	"	"	133	1.21	"	5.7	3.74
7	+	"	"	130	1.47	"	8.4	3.10
8	-	"	"	133	1.47	"	7.3	3.64
9	+	"	1.24	121	1.32	2.22	9.4	2.26
10	-	"	"	130	1.32	"	7.2	3.16
11	+	"	"	125	1.26	2.17	7.6	2.70
12	-	"	"	126	1.26	"	5.7	3.64
13	+	2.85	1.50	125	1.19	2.23	6.5	2.82
14	-	"	"	129	1.19	"	5.0	3.78
15	-	"	"	125	1.11	2.19	5.85	2.82
16	+	"	1.43	127	1.08	2.35	7.3	2.46
17	-	"	"	128	1.08	"	5.4	3.32
18	-	"	"	129	1.08	"	5.3	3.42
19	+	3.22	1.80	123	1.13	2.44	5.1	3.30
20	-	"	"	124	1.13	"	4.9	3.48
								3.19×10^{10}

The sudden variations in the values of $1/\beta$ are due to changes purposely made in the needle.

The last column gives the values of V . This work is not intended as a determination of V , but the calculation is made merely to show to what degree of approximation the effect follows the assumed law.

The deflexions are about the same as those obtained in the Berlin experiments—5 to 8 millim. on reversing. The force measured then was $1/50000$ H ; now it is $1/125000$ H. The sensitiveness of the needle in the two cases was almost the same. In the former experiment a force of 3×10^{-7} deflected the needle $1'$ of arc ; the corresponding number now is 2.7×10^{-7} , slightly more sensitive. The scale distances were 110 and 200 centim. respectively. So this experiment gives about the same scale-deflexion at twice the distance with a force $\frac{4}{10}$ as great. The agreement between the two is seen to be quite good.

The observations, except Nos. 1, 2, 15, and 18 given above, were taken in pairs—first one direction of rotation and the other immediately afterwards, everything except the rotation being kept constant.

The table shows that, in every case except one, the deflexion for negative rotation is appreciably smaller than the corresponding positive.

The difference is too great to be due to accidental errors in the readings, as the following table, giving the successive deflexions in the case of #13 and #14 will show.

TABLE II.

#13.	#14.
mm.	
6.7	5.1
5.1	4.9
6.6	3.9
7.6	5.2
8.0	5.0
5.8	5.2
6.3	4.9
8.0	5.0
8.0	5.0
4.3	4.4
5.9	6.6
6.0	5.0
6.5	5.0

There is but one deflexion in #13 as small as the mean of #14, and but one in #14 as large as the mean of #13.

This is a fair example of the way the deflexions run. As a further illustration of this take #17 and #18; these two are identical in arrangement, but the direction of rotation is in one case got by crossing the belts from the countershaft to the disks and leaving the main belt straight; in the other the main belt is crossed while the auxiliary belts are straight. The deflexions are the same. This, too, shows that the difference cannot be due to any effect of the countershaft. The cause of this has not yet been explained. The work is to be continued with this and also with new apparatus, made like the Berlin apparatus, but with the disk much larger, 30 centim. in diameter; at least double the speed then obtained will be used. This ought to give deflexions on reversal of 1.5 to 1.7 centim.

The values of V do not agree so well as might be looked for; but when, in addition to the numerous difficulties already mentioned, the smallness of the deflexion is considered, and the possibility of the needle being affected by currents or magnets in other portions of the laboratory, so far away as not to be guarded against, and which might well be changed between the time of taking the observation and the determination of the needle-constant, and, finally, that a disturbing cause of some kind is still undoubtedly present, the agreement is seen to be as good as could justly be expected.

Physical Laboratory,
Johns Hopkins University,
April 22, 1889.

NOTE, added April 29.

There seems to be a misunderstanding in certain quarters as to the nature of the deflexion obtained in Prof. Rowland's first experiment. The paper reads:—"The swing of the needle on reversing the electrification was about 10 to 15 mm., and therefore the point of equilibrium was altered 5 to 7.5 mm." This has been construed to mean that the deflexion was merely a throw, and that no continuous deflexion was obtained. This is entirely erroneous; there was always a continuous deflexion. The throw was read merely because the needle was always more or less unsteady, and better results could be got by seizing a favourable moment when the needle was quiet and reading the throw, than by attempting to take the successive elongations, or waiting for the needle to come to rest. In the experiment described above the needle was very steady and no such trouble was

experienced. On electrifying, the needle would take up a certain position and would remain there as long as the charge was kept up; on reversal, it would move off to a new and perfectly definite position about 6 to 7 mm. away, and remain there, &c.

H. A. R.

C. T. H.

LIII. *On the Character of the Complete Radiation at a given Temperature.* By LORD RAYLEIGH, *Sec. R.S., Professor of Natural Philosophy in the Royal Institution**.

BY complete radiation is here meant the radiation which would ultimately establish itself in an enclosure, whose walls are impervious, and are maintained at a uniform temperature. It was proved by Stewart and Kirchhoff that this radiation is definite, not only in the aggregate, but also in its various parts; so that the energy radiated with wave-frequencies between n and $n + dn$ may be expressed by

$$F(n) dn, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where, for a given temperature, $F(n)$ is a definite function of n . The reservation implied in the word *ultimately* is necessary in order to exclude radiation due to phosphorescence or to chemical action within the enclosure. The radiation commonly characterized, so far at any rate as its visible elements are concerned, by the term *white*, is supposed to be approximately similar to the complete radiation at a certain very high temperature.

As remarked by Kirchhoff, the function F , being independent of the properties of any particular kind of matter, is likely to be of a simple form; and speculations have naturally not been wanting. Within the last two years the subject has been considered by W. Michelson† and by H. F. Weber‡. The former, on the basis of an *à priori* argument of a not very convincing character, arrives at the conclusion that at temperature θ the radiation between the limits of wave-length λ and $\lambda + d\lambda$ may be expressed

$$I_{\lambda} d\lambda = B \theta^{-\frac{5}{2}} f(\theta) e^{-\frac{c}{\theta \lambda^2}} \lambda^{-2p-4} d\lambda. \quad . \quad . \quad . \quad (2)$$

According to Stephan the total radiation is proportional to θ^4 .

* Communicated by the Author.

† *Journal de Physique*, t. vi. Oct. 1887; *Phil. Mag.* xxv. p. 425.

‡ *Berlin Sitz. Ber.* 1888.

In conformity with this Michelson supposes that

$$p=1, \quad f(\theta)=K\theta^3;$$

so that (2) assumes the more special form

$$I_\lambda=B_1\theta^3e^{-\frac{c}{\theta\lambda^2}}\lambda^{-6}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If, as appears to be preferable, we take n as independent variable, $F(n) \, dn$ is of the form

$$Ae^{-a^2n^2}n^4 \, dn, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

A, a being functions of θ , but independent of n .

Weber's formula, so far as it here concerns us, is of a still simpler character. Expressed in terms of n , it differs from (4) merely by the omission of the factor n^4 , thus corresponding to $p=-1$ in (2); so that

$$F(n) \, dn=Ae^{-a^2n^2} \, dn. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The agreement between (5) and the measurements by Langley of the radiation at 178° C. is considered by Weber to be sufficiently good.

In contemplating such a formula as (5), it is impossible to refrain from asking in what sense we must interpret it in accordance with the principles of the Undulatory Theory, and whether we can form any distinct conception of the character of the vibration indicated by it. My object in the present paper is to offer some tentative suggestions towards the elucidation of these questions.

The first remark that I would make is that the formula must not be taken too literally. If there is one thing more certain than another, it is that a definite wave-frequency implies an infinite and unbroken succession of waves*. A good illustration is afforded by intermittent vibrations, as when a sound itself constituting a pure tone is heard through a channel which is periodically opened and closed. Such an intermittent vibration may be represented by†

$$2(1 + \cos 2\pi mt) \cos 2\pi nt, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where n is the frequency of the original vibration, and m the frequency of intermittence. By ordinary trigonometrical transformation (6) may be written

$$2 \cos 2\pi nt + \cos 2\pi(n+m)t + \cos 2\pi(n-m)t; \quad . \quad (7)$$

* "The pitch of a sonorous body vibrating freely cannot be defined with any greater closeness than corresponds with the total number of vibrations which it is capable of executing." (Proc. Mus. Assoc. Dec. 1878, p. 25.)

† "Acoustical Observations," III., Phil. Mag. April 1880.

which shows that in this case the intermittent vibration is equivalent to *three* simple vibrations of frequencies n , $n+m$, $n-m$.

In order to distinguish wave-frequencies, whose difference is small, a correspondingly long series of waves is necessary; and of no finite train of irregular vibrations can it be said that waves of a certain frequency are present, and waves of a frequency infinitely little different therefrom absent. Neither can the proportions in which the two are present be assigned. In professing to assign these proportions, (5) and similar formulæ make assertions not directly supported by experiment. In a sense all the formulæ of mathematical physics are in this predicament; but here the assertion is of such a nature that it could not be tested otherwise than by experiments prolonged over all time.

In practice it is not time that brings the limitation, but the resolving power of our instruments. In gratings the resolving power is measured by the product of the total number of lines and the order of the spectrum under examination*. It will be allowing a good deal for the progress of experiment if we suppose that in measurements of energy it may be possible to discriminate wave-lengths (or frequencies) which differ by a millionth part. But a million wave-lengths of yellow light would occupy only 60 centim., and the waves would pass in 2×10^{-9} seconds! Waves whose frequencies differ by less than this are inextricably blended, even though we are at liberty to prolong our observations to all eternity.

At any point in the spectrum of a hot body there are, therefore, mingled waves of various frequencies lying within narrow limits. The resultant for any *very short interval of time* may be identified with a simple train, whose amplitude and phase, depending as they do upon the relative phases of the components, must be regarded as matters of chance. The probability of various amplitudes depends upon the principles explained in a former communication, "On the Resultant of a large number of Vibrations of the same Pitch and of Arbitrary Phase."† After an interval of time comparable with 10^{-9} second the amplitude is again practically a matter of chance; so that during the smallest interval of time of which our senses or our instruments could take cognizance, there are an immense number of independent combinations. But, under these circumstances, as was shown in the place referred to, we have to do merely with the sum of the individual intensities.

* Phil. Mag. vol. xlvii. p. 200 (1874).

† Phil. Mag. Aug. 1880.

In his excellent memoir, *Sur le mouvement lumineux**, M. Gouy suggests that the nature of white light may be best understood by assimilating it to a sequence of entirely irregular impulses. It was by means of this idea that Young† explained the action of gratings; and although J. Herschel‡ took exception, there is no doubt that the method is perfectly sound. The question that I wish to raise is whether it is possible to define the kind of impulse of which an irregular sequence would represent the complete radiation of any temperature.

The first thing to be observed is that it will not do to suppose the impulses themselves to be arbitrary. In proof of this it may be sufficient to point out that in that case there would be no room for distinguishing the radiations of various temperatures. If the velocity at every point were arbitrary, that is independent of the velocity at neighbouring points however close, the radiation could have no special relation to any finite wave-length or frequency. In order to avoid this discontinuity we must suppose that the velocities at neighbouring points are determined by the same causes, so that it is only when the interval exceeds a certain amount that the velocities become independent of one another. This independence enters gradually. When the interval is very small, the velocities are the same. As the interval increases, the arbitrary element begins to assert itself. At a moderate distance the velocity at the second point is determined in part by agreement with the first, and in part independently. With augmenting distance the arbitrary part gains in importance until at last the common element is sensibly excluded§.

* *Journ. de Physique*, 1886, p. 354. I observe that M. Gouy had anticipated me (*Enc. Brit.* xxiv. p. 425) in the remark that the production of a large number of interference-bands from originally white light is a proof of the resolving power of the spectroscope, and not of the regularity of the white light. It would be instructive if some one of the contrary opinion would explain what he means by regular white light. The phrase certainly appears to me to be without meaning—what Clifford would have called *nonsense*.

† *Phil. Trans.* 1801.

‡ *Enc. Metrop.*, Light, § 703 (1830).

§ The following may serve as an illustration. Out of a very large number of men (say an army) let a regiment of 1000 be chosen by lot, and let the deviation of the mean height of the regiment from that of the army be exhibited as the ordinate of a curve. If a second set of 1000 be chosen by lot, the new ordinate will bear no relation to the old. But if at each step but one man of the regiment be eliminated by lot, and one successor be chosen in the same way, the new ordinate will be almost the same as the old one, and not until after a large number of steps (of the order of 1000) will the new ordinate become sensibly independent. If the abscissa be taken proportional to the number of steps (each finally

Now this is precisely the condition of things that would result from the arbitrary distribution of a large number of impulses, in each of which the medium is disturbed according to a defined law. A simple case would be to suppose that each impulse is confined to a narrow region of given width, and within that region communicates a constant velocity*. An arbitrary distribution of such impulses over the whole length would produce a disturbance having, in many respects, the character we wish. But it is easy to see that this particular kind of impulse will not answer all requirements. For in the result of each impulse, and therefore in the aggregate of all the impulses, those wave-lengths would be excluded, which are submultiples of the length of the impulse. The objection could be met by combining impulses of different lengths; but then the whole question would be again open, turning upon the proportions in which the various impulses were introduced. What I propose here to inquire is whether any definite type can be suggested such that an arbitrary aggregation of them will represent complete radiation. It will be evident that in the definition of the type a constant factor may be left arbitrary. In other words, the impulses need only to be *similar*, and not necessarily to be *equal*.

Probably the simplest type of impulse, $\phi(x)$, that could at all meet the requirements of the case is that with which we are familiar in the theory of errors, viz.

$$\phi(x) = e^{-c^2 x^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

It is everywhere finite, vanishes at an infinite distance, and is free from discontinuities. A single impulse of this type may be supposed to be the resultant of a very large number of localized infinitesimal simultaneous impulses, all *aimed* at a single point ($x=0$), but liable to deviate from it owing to accidental causes. I do not at present attempt any physical justification of this point of view, but merely note the mathematical fact. The next step is to resolve the disturbance (8) into its elements in accordance with Fourier's theorem. We have

treated as infinitesimal), the resulting curve will have the required property, and would exhibit a possible form for complete radiation. It seems not unlikely that the law is here the same as that obtained below on the basis of (8).

* The reader may fix his ideas upon a stretched string vibrating transversely.

$$\begin{aligned}\phi(x) &= \frac{1}{\pi} \int_0^\infty \int_{-\infty}^{+\infty} \cos u (v-x) \phi(v) du dv \\ &= \frac{1}{\pi} \int_0^\infty \int_{-\infty}^{+\infty} \cos uv \cos ux e^{-c^2 v^2} du dv. \quad . \quad . \quad (9)\end{aligned}$$

Now

$$\int_{-\infty}^{+\infty} e^{-c^2 v^2} \cos uv dv = \frac{\sqrt{\pi}}{c} e^{-u^2/4c^2}; \quad . \quad . \quad . \quad (10)$$

so that

$$e^{-c^2 x^2} = \frac{1}{c\sqrt{\pi}} \int_0^\infty e^{-u^2/4c^2} \cos ux du. \quad . \quad . \quad . \quad (11)$$

This equation exhibits the resolution of (8) into its harmonic components; but it is not at once obvious how much energy we are to ascribe to each value of u , or rather to each small range of values of u . As in the theory of transverse vibrations of strings, we know that the energy corresponding to the product of any two distinct harmonic elements must vanish; but the application of this, when the difference between two values of u is infinitesimal, requires further examination. The following is an adaptation of Stokes's investigation* of a problem in diffraction.

By Fourier's theorem (9) we have

$$\pi \cdot \phi(x) = \int_0^\infty f_1(u) \cos ux du + \int_0^\infty f_2(u) \sin ux du, \quad . \quad . \quad (16)$$

where

$$f_1(u) = \int_{-\infty}^{+\infty} \cos uv \phi(v) dv, \quad . \quad . \quad . \quad (17)$$

$$f_2(u) = \int_{-\infty}^{+\infty} \sin uv \phi(v) dv. \quad . \quad . \quad . \quad (18)$$

In order to shorten the expressions, we will suppose that, as in (11),

$$f_2(u) = 0.$$

We have

$$\pi^2 \cdot \{\phi(x)\}^2 = \int_0^\infty \int_0^\infty f_1(u) f_1(u') \cos ux \cos u'x du du'.$$

This equation is now to be integrated with respect to x from $-\infty$ to $+\infty$; but, in order to avoid ambiguity, we will introduce the factor $e^{\mp \alpha x}$, where α is a small positive quantity. The positive sign in the alternative is to be taken when x is

* Edinb. Trans. xx. p. 317 (1853); see also *Enc. Brit.* t. xxiv. p. 431.

negative, and the negative sign when x is positive. The order of integration is then to be changed, so as to take first the integration with respect to x ; and finally α is to be supposed to vanish. Thus

$$2\pi^2 \cdot \{\phi(x)\}^2 = \text{Lim.} \int_{-\infty}^{+\infty} \int_0^\infty \int_0^\infty e^{\mp \alpha x} f_1(u) f_1(u') \{\cos x(u' - u) + \cos x(u' + u)\} du du' dx.$$

Now

$$\int_{-\infty}^{+\infty} e^{\pm \alpha x} \cos hx dx = \frac{2\alpha}{\alpha^2 + h^2};$$

so that

$$2\pi^2 \{\phi(x)\}^2 = \text{Lim.} \int_0^\infty \int_0^\infty \left\{ \frac{2\alpha}{\alpha^2 + (u' - u)^2} + \frac{2\alpha}{\alpha^2 + (u' + u)^2} \right\} f_1(u) f_1(u') du du'. \quad (19)$$

Of the right-hand member of (19) the second integral vanishes in the limit, since u and u' are both positive quantities. But in the first integral the denominator vanishes whenever u' is equal to u . If we put

$$u' - u = \alpha z, \quad du' = \alpha dz,$$

then, in the limit

$$\int_0^\infty \frac{2\alpha f_1(u') du'}{\alpha^2 + (u' - u)^2} = \int_{-\infty}^{+\infty} \frac{2f_1(u) dz}{1 + z^2} = 2\pi f_1(u)$$

Thus

$$\int_{-\infty}^{+\infty} \{\phi(x)\}^2 dx = \frac{1}{\pi} \int_0^\infty \{f_1(u)\}^2 du. \quad (20)$$

If $f_2(u)$ be finite, we have, in lieu of (20),

$$\int_{-\infty}^{+\infty} \{\phi(x)\}^2 dx = \frac{1}{\pi} \int_0^\infty [\{f_1(u)\}^2 + \{f_2(u)\}^2] du. \quad (21)$$

In M. Gouy's treatment of this question, the function $\phi(x)$ is supposed to be ultimately periodic. In this case $f(u)$ vanishes whenever u differs from one or other of the terms of an arithmetical progression; and the whole kinetic energy of the motion is equal to the sum of those of its normal components, as in all cases of vibration. The comparison of this method with the one adopted above, in which all values of u occur, throws light upon the nature of the harmonic expansion.

It is scarcely necessary to point out that vibrations started

impulsively from rest divide themselves into two groups, constituting progressive waves in the two directions, and that the whole energy of each of these waves is the half of that communicated initially to the system in the kinetic form*.

The application of (21) to (11), where

$$f_1(u) = \frac{\sqrt{\pi}}{c} e^{-u^2/4c^2},$$

gives

$$\int_{-\infty}^{+\infty} e^{-2c^2x^2} dx = \frac{1}{c^2} \int_0^{\infty} e^{-u^2/2c^2} du, \quad . \quad . \quad (22)$$

as may be easily shown independently. The intensity, corresponding to the limits u and $u + du$, is therefore

$$e^{-2} e^{-u^2/2c^2} du;$$

and this, since u and n are proportional, is of the form (5).

If an infinite number of impulses, similar (but not necessarily equal) to (8), and of arbitrary sign, be distributed at random over the whole range from $-\infty$ to $+\infty$, the intensity of the resultant for an absolutely definite value of n would be indeterminate. Only the *probabilities* of various resultants could be assigned. And if the value of n were changed, by however little, the resultant would again be indeterminate. Within the smallest assignable range of n there is room for an infinite number of independent combinations. We are thus concerned only with an average, and the intensity of each component may be taken to be proportional to the total number of impulses (if equal) without regard to their phase-relations. In the aggregate vibration, the law according to which the energy is distributed is still for all practical purposes that expressed by (5).

If we decompose each impulse (8) in the manner explained, we may regard the whole disturbance as arising from an infinite number of simultaneous elementary impulses. These elementary impulses are distributed not entirely at random; for they may be arranged in groups such that the members of each group are of the same sign, and are, as it were, aimed at the same point under a law of error; while the different groups are without relation, except that the law of error is the same for all. It is obviously not essential that the different groups should deliver their blows simultaneously. Further, it would have come to the same thing had we supposed all the impulses to be delivered at the same point

* 'Theory of Sound,' vol. ii. § 245.

in *space*, but to be distributed in *time* according to a similar law. In comparing the radiations at various temperatures, we should have to suppose that, as the temperature rises, not only does the total number of elementary impulses (of given magnitude) increase, but also the *accuracy of aim* of each group.

We have thus determined a kind of impulse such that an arbitrary aggregation of them will represent complete radiation according to Weber's law (5). One feature of this law is that $F(n)$ approaches a finite limit as n decreases. In this respect W. Michelson's special law (4) differs widely; for, according to it, $F(n)$ vanishes with n . This evanescence of $F(n)$ implies that the integrated value of each of our component impulses is zero. If we wish to inquire further into the law of the impulse, we have to determine $\phi(x)$ so that

$$f_1(u) = C u^2 e^{-u^2/4c^2} \dots \dots \dots (23)$$

By successive differentiations of (10) with respect to u , it may be shown that

$$\frac{\sqrt{\pi}}{c} \frac{u^2}{2c^2} e^{-u^2/4c^2} = \int_{-\infty}^{+\infty} e^{-c^2 x^2} (1 - 2c^2 x^2) \cos ux dx \dots (24)$$

Thus, if we take

$$\phi(x) = e^{-c^2 x^2} (1 - 2c^2 x^2), \dots \dots \dots (25)$$

$f_1(u)$ will be of the required form. The curve representative of (25), viz.

$$y = e^{-x^2} (1 - 2x^2), \dots \dots \dots (26)$$

is symmetrical with respect to $x=0$, vanishes when $x = \pm \infty$ and also when $x = \pm 2^{-\frac{1}{2}}$. The positive area between the last-named limits is numerically equal to the negative area lying outside them.

Other proposed forms for $f(u)$, such as those included in (2), might be treated in a similar way; but the above examples may suffice. The simplicity of (8) compared, *e. g.*, with (25), may be regarded as an argument in its favour. But we do not know enough of the mechanism of radiation to draw any confident conclusion. What we most require at present is more complete data from experiment, such as have been promised by Prof. Langley. As regards the radiation of very low frequency, a question may arise as to whether it is included in our present measurements. Some authorities have favoured the view that, when the frequency is sufficiently diminished, all kinds of matter become transparent; but the electric theory seems to point in the opposite direction.

In comparing any theoretical formula with experiment, we must not forget that what we learn directly from the latter is the *difference* of radiations at *two* temperatures.

One more remark in conclusion. If the complete radiation for a given temperature be represented by (5), it follows that temperature may be defined by the value of a . The contrary would imply that the law of distribution is the same at all temperatures, and would be inconsistent with ordinary observation respecting "red" and "white heats." Now the dimensions of a are those of a time; so that temperature may be defined by a *time*, or (through the velocity of propagation) by a *line*. Thus in Prof. Langley's curves, which represent the distribution of energy in a diffraction spectrum, the wavelength corresponding to the maximum ordinate may be regarded as a linear specification of the temperature to which the curve relates.

Terling Place, Witham, Essex,
April 24, 1889.

LIV. *On an Electrostatic Field produced by varying Magnetic Induction.* By Dr. OLIVER LODGE*.

IN common no doubt with many others, I have long wished to find some connexion between static electricity and magnetism. My early notions in this direction were such ideas as the following:—

(1) To spin a long bar-magnet on its axis, suspending in its field a sort of quadrant-electrometer needle charged oppositely at either end, and to look for a deflexion. The iron core of an electromagnet was supposed to serve.

(2) To construct a cylindrical or drum-shaped horseshoe-magnet like two iron wheels joined at the nave, and wound with wire there; to spin this on its axis and suspend a charged pith-ball between the two rims †.

(3) To use a varying magnet instead of a moving one [quite probably this was suggested by Prof. Fitzgerald in a conversation I had with him some ten years back]; for instance, to suspend a charged gold leaf between the edges of the pole-pieces of an electromagnet, and watch it through a microscope when the exciting current is started or stopped.

Many other varieties of the experiment are in my notebooks, but these are enough to show the idea.

* Communicated by the Physical Society: read May 11, 1889.

† I find, on reference to Dr. S. P. Thompson's book, that one may describe this arrangement as the field-magnet of a Mordey dynamo.

The importance of the experiment seemed to me (rightly or wrongly) to lie in a possible crucial criterion between Maxwell's theory and some of the contemporary German theories; because it seemed to deal with something less than an open circuit, viz. no circuit at all.

It also struck me as a sort of converse experiment to that which Prof. Rowland has recently and so successfully carried out: the classical experiment of deflecting a magnetic needle by a rotating charged disk.

This can hardly be considered a connexion between electrostatics and magnetism, because a static charge in motion, if not a contradiction in terms, is essentially a current; and what the experiment proves is the truth of Maxwell's view that all electricity in motion, no matter whether by conduction, displacement, or convection, is a true current, and exerts magnetic effects. I should rather say that Rowland's experiment proves the truth of this view of Maxwell so far as convection is concerned. The question of displacement is a ticklish one just now, upon which I shall be safer to be silent*.

But, now, think of a charged gold leaf hanging between the poles of a magnet. If the gold leaf move it constitutes a current, and therefore at once feels a force urging the current (*i. e.* the line of motion) across the lines of force in the orthodox way. Instead of supposing the gold leaf to move, let the magnet move. The same thing will happen, because the relative motion is the same as before. (If absolute motion of the static charge through æther is essential, then this last statement falls flat and may possibly be false.)

If this force were observed, it would be a connexion between magnetism and a static charge, provided any motion of the charged body due to the earth's motion in its orbit were shown to be inoperative or unessential.

Or, instead of moving the magnet, let its strength be varied; lines of force pass by the charged body, unless it is symmetrically situated, in this case as in the other; and hence the effect may naturally be expected to be the same in either case.

The experiment I wished to try was therefore not exactly a converse of the Rowland experiment, but was closely related to it.

Two years ago my friend Mr. A. P. Chattock came to

* I say this with reference to what went on at Bath, not with reference to anything that has happened since. This paper was written two or three months ago, before I had heard anything of a communication from Dr. S. P. Thompson to the Royal Society.

Liverpool to act as Demonstrator, and I found him imbued with similar notions, and also with more practical ideas for carrying them out. Especially was he convinced of the fact that an actual magnetic field in which to suspend the charged body was entirely superfluous, but that varying magnetic induction in a region near the charged body would be equally effective and easier to manage. He wished to use, in fact, a closed magnetic circuit with all its lines enwrapped in itself, and to hang the charged body near but outside the circuit, so that no lines of magnetic force really ever passed through the charge, at least when the magnetic induction was steady. But when the magnetic induction was varied, say by reversing a magnetizing current round the closed magnetic circuit or ring solenoid, Mr. Chattock pictured lines flashing past the charged body in enormous numbers and producing the desired effect.

I am bound to say that the idea of the closed magnetic circuit was not natural to me at first, and I resisted the views of the inventor of the magnetic potentiometer for the best part of a week. At the end of that time my conversion had taken place; and from that time to this some sort of closed magnetic circuit, with one or other form of charged body inside it, has been set up in my laboratory and experiments made with it from time to time.

The theory of the effect I have observed it may be useful just to write down in the most elementary manner.

The magnetic induction in a solenoid being I , the E.M.F. induced in any complete circuit round it, when I changes, is

$$e = \frac{dI}{dt} \dots \dots \dots (1)$$

If an E.M.F. act on a statically charged body at a distance r from centre of solenoid, the work done in driving it once round a circle is

$$eQ = 2\pi r F, \dots \dots \dots (2)$$

where F is the mechanical force exerted on the charged body.

Now if the E.M.F. in (2) is the same as in (1), and is caused by the induction I being either generated or destroyed, it will only last a very short time, and we must therefore consider what is the whole impulse of the force during the time it lasts, viz.

$$\phi = \int_0^\infty F dt = \frac{IQ}{2\pi r} \dots \dots \dots (3)$$

And this is the momentum generated whenever the solenoidal magnetism is generated or destroyed. This therefore, or some simple multiple of it, is what is observed in the experiment.

Now in calculating out a numerical value for this we shall write

$$I = \frac{4\pi n C A \mu}{l} = \mu C \text{ times a length,}$$

and

$$Q = SV = KV \text{ times a length.}$$

Hence in the product IQ occurs the product μK , and this is the reciprocal of the square of the velocity of light.

This amply accounts for the smallness of the observed effect. By various devices the geometrical parts of I and Q can be increased somewhat to make the observation more easy, but nothing can get over the fact that the velocity of light squared occurs in the denominator of the expression.

To return to the account of experiments devoted to finding this effect. If there is any importance in the result, and I hope there is, though I sometimes fear there is not, much of the credit belongs to Mr. Chattock. The only reason why this is not a joint paper is because he had left the laboratory before a result was obtained.

I must mention also that he showed me, somewhere about this same time, an ingenious paper read before the Society of Telegraph Engineers some two or three years back by Mr. George Forbes, in which that gentleman, making free use of the analogy between a magnetic circuit and a voltaic circuit, predicted almost the precise arrangement which we have found successful, as a magnetic analogue of a tangent-galvanometer.

Some other predictions were also made on the strength of the same analogy ; but I have made no experiments like them at present.

The experiment of which I have to speak may be described as follows :—You buy at a shop a large hank of iron wire weighing 28 lbs. or so : you cover it with tape and wind it over with No. 14 copper wire connected to a reversing-key and storage battery. At the centre of the ring you place a glass vessel in which are delicately suspended two oppositely-charged very light conductors, made, for instance, of aluminium foil, connected to each other and to a mirror by a shellac arm. You then work the reversing-key by hand in unison with the natural period of vibration of the suspended arm, so as to keep on reversing the magnetism in the ring, and you hope thus

o get up a microscopic swing in the arm or to check an already existing one.

Under these circumstances a result may very well be observed; but it will be certainly due to a spurious cause unless far more elaborate precautions are taken.

For a whole day, last Christmas twelvemonth, I got the most beautifully consistent results. The deflexion reversed with the current, with the sign of the static charge, and everything just as it ought. But before jumping to any conclusion the precaution was taken of going through the operations with the key while one end of the wire was disconnected and the battery circuit therefore not complete. The effect was much stronger.

The whole thing was a mere electrostatic effect due to the action of the slope of potential in the conducting-wire on the very sensitive electrometer-like needle.

To get over this effect was very troublesome and seemed impossible. A number of obvious things were tried, such as cutting the wire in the middle and adjusting its two halves symmetrically; putting the middle of the wire to earth, &c. Putting a carefully selected portion of the wire to earth did, indeed, get over the difficulty; but so sensitive was the arrangement that a variation of an inch in the position of the earth-contact on the length of the wire made a perceptible difference. The effect seemed altogether larger than what we wanted to observe; so, at length, I had the whole glass box containing the charged needle coated with tinfoil.

The result was a beautiful steadiness. The slope of potential was utterly cut off and not a trace of oscillation could be detected on open circuit.

But then neither could anything be detected when the circuit was closed. If the needle were at rest, it remained at rest; and if it were in motion, its motion subsided without the least reference to what one happened to be doing with the key. I came sorrowfully to the conclusion that in cutting off the electrostatic disturbance we had neutralized the thing we were looking for.

But on writing this view to Mr. Chattock, I found he did not agree with it. His view was that the magnetic lines had to get through the space in the centre of the coil, and that, though a sheet of tinfoil might delay them momentarily, it could make no difference in the end; since the time required for the reversal of the split-up mass of iron was far less than that of a semi-oscillation of the needle.

Not feeling absolutely sure either way, I determined, at

any rate, to try a more sensitive arrangement. Hitherto the suspending-fibre had been horizontal, and the time of oscillation fairly quick, the mirror, as it were, lying on its back, and the light-beam reflected down to it by a shaving-glass.

All this was now changed. The iron ring was set up vertically, and the needle was delicately suspended, care being taken to keep its moment of inertia very small. The ring was wound, not all over, but only over a sector of some 45° , and this wire-covered portion was then carefully screened by sheet copper from being able to see the needle and thereby affect it electrostatically. The copper did not form a closed circuit.

The needle was again a pair of scraps of aluminium-foil attached to the ends of a shellac arm holding also a mirror. The charging was done after a proof-plane fashion, and the insulation was pretty good. Asbestos and sulphuric acid kept the air of the box dry.

Taking suitable precautions and trying the magnetic experiment under the new conditions, with the electrostatic effect of the wire carefully screened off, but no other part of the iron ring screened from the needle, an effect was observed, and it was small. But it was irreversible.

I traced it, after a little difficulty, to Foucault currents set up in the aluminium scraps. One might certainly have expected such currents there; but I did not expect them to matter, except by reason of heat, because the metal is in no magnetic field—except, indeed, the earth's. However, it must be that some few stray lines escape from the iron circuit and, crossing through the air, affect the currents excited in the metal by the changing magnetic induction.

So I had to abandon aluminium, and selected very thin mica, silvered chemically, and held curved to the proper curvature by silk threads. But even in the thin silver film Foucault currents could still be detected; so the film was scored regularly all over with a needle-point. But though this diminished the effect to almost nothing, it was still comparable to what we wanted to observe.

So I beat about for other conductors, good enough to charge statically, but bad enough not to have currents induced in them. At length I hit upon the thin gelatine stuff which is wrapped round crackers. It was not exactly this, but it was some variety of gelatine that Davies ultimately used, constructing it into a pair of little cylinders, say $\frac{1}{8}$ inch long by $\frac{1}{8}$ inch in diameter, very light, and only just conducting enough to receive a charge after they had been in the artificially dried air of the glass box for some time. The shellac arm and everything were made afresh, and as slight and fragile as possible.

I also proceeded to construct a larger and more carefully made iron ring, instead of a mere hank as bought. I thought of making a figure-of-8 ring, winding one loop with wire, and hanging the needle in the other loop; but practical difficulties postponed the adoption of this plan, a circular ring being so much easier; so I went on with the notion of winding the copper wire only over a sector, and screening that by copper sheet. To make this ring an enormous quantity of fine iron wire was procured, and wound for a great part of a month on a suitable wooden mould in a slow-motion lathe, the wire being passed through a flame and through a stick of shellac on its way, so as to roughly insulate its turns, in case we wanted to employ rapid reversals.

The winding continued until the wooden mould began to break down, when it was extracted, all matted together, and mounted on a stone pillar standing on rock in the laboratory.

Some natural difficulty is found in getting the charged needle to hang as wished, with its mirror in the right aspect. Some difficulty is also found in arranging that the stability of the needle shall not be too much increased by its electric charge, so that it oscillates violently about some point and is not properly slow and sensitive.

Another and very tedious difficulty is to arrange so that the position the needle is willing to take up before and after charge, is nearly the same; for if the two be very different the spot of light will not keep still, but will sail steadily along as the charge slowly leaks away. To detect small effects the spot of light must be very still. It was difficult to secure this without at the same time applying too severe a constraint, either electric or other, to the needle. It had to be done pretty much by selecting carefully the shape to be given to the curved plates of the needle, and to the enclosure opposed to them.

All these difficulties were gradually more or less overcome through the patience and skill of my laboratory assistant, Mr. Benjamin Davies.

There remained a few irregular disturbances, some of which could be traced to convection-currents, others to the ordinary movements about a building, and others again to the passage of London and North-Western trains in their tunnel, some 150 yards away and 60 feet down, in the sandstone rock.

The laboratory at Liverpool is very favourably situated as regards shaking. It is a substantial stone building, with walls 2 feet thick, on sandstone rock, which is partially isolated from the railway by the remains of an old quarry, which has been filled with rubble. Street traffic is very distant, and has never been appreciably felt to my knowledge. Students

tramping about in the daytime appear to be the cause of most of such irregularities as are felt ; but this cause is happily absent at certain hours of the day (or night), and there remains literally nothing but the passage of the North-Western trains, and these are distinctly felt, though only with the most delicate means. The most serious shaking is during a gale. The building then certainly quivers.

To illustrate the unusual steadiness, it may be interesting to mention that Mr. Chattock rigged up the Cavendish Gravitation experiment in my lecture-theatre, taking such precaution as I naturally suggested about suspending from a ball of lead hung from the wall by elastic ; and twice I have successfully exhibited the experiment to a fairly large audience, the first time very well indeed. Convection-currents are the real trouble in this experiment, and these can, by elaborate tinfoil screens, be nearly avoided*.

After a few preliminary experiments with the new ring and needle, I began seriously to mistrust the effect of the copper near the ring. Sheet copper was used to screen off the wire from the needle, and it was also used all over the outside of the arrangement to screen off stray electrostatic actions around. But I fancied I understood now what the effect of such conductors was. The motion of the electricity in them was just of the same sign as the motion of the electrostatic charge for which we were looking ; and if the motion by conduction were permitted, the reaction of the charge thus momentarily redistributed on the conducting metal might just mask the direct effect of the same electromotive force on the charged body ; and would mask it, in so far as it was suitably placed for masking it.

Metals do not screen off the magnetic effect of a moving electrostatic charge (*vide* Rowland), but they might screen off the converse electrostatic effect of a varying magnetic induction.

On the other hand, by arranging a copper conductor so that the effect of electric charges induced in it by the E.M.F. should *assist* the direct effect of the E.M.F. on the charged body, then no doubt a deflexion might be rendered visible.

So I led a short wire round the outside of the ring, and brought its ends into the box where the needle was, one on each side of the needle.

* Since seeing Mr. Boys's beautiful pocket-arrangement for exhibiting the same effect at the Royal Society soirée this week, I am out of conceit with this apparatus. Evidently the experiment is going to become a commonplace of the class-room.

The only disturbance now found was due to heat from the wire below the glass box containing the needle, and to get over this Davies rigged up a number of concentric cylindrical jackets of tin plate round the box containing the needle. These certainly constituted an electrostatic screen. Nevertheless, I had hope that though such a screen might diminish the effect it would not destroy it.

I had a letter from Davies yesterday saying that a very minute effect could be seen with the screens all on, and that it did everything properly as regards reversals. On taking off one screen it increased. On taking off another it increased more, but showed signs of superposed spuriousness. On taking off the third screen the effect was masked by spurious actions.

Everything points to the fact, therefore, that we have now several times observed the true effect, and I entertain practically no doubt of it.

But inasmuch as some of the observations have been made so lately, and some even since I left Liverpool this time, I would not wish the Society to suppose that I regard the research as finished and complete. I should like to clear up distinctly the effect of screens.

I must speak in high praise of the skill and neat-fingeredness of my laboratory assistant Mr. Davies in carrying out the rather troublesome requirements which this finnickish research has at various times seemed to necessitate.

LV. *Table of Standard Wave-Lengths.*

By Professor H. A. ROWLAND*.

IN the 'American Journal of Science' for March, 1887, and the Phil. Mag. for the same date, I have published a preliminary list of standards as far as could be observed with the eye, with a few imperfectly observed by photography, the whole being reduced to Bell's and Peirce's values for absolute wave-lengths. Mr. Bell has continued his measurements and found a slightly greater value for the absolute wave-length of the D line, and I have reduced my standards to the new values.

Nearly the whole list has been gone over again, especially at the ends around the A line and in the ultra-violet. The wave-lengths of the ultra-violet were obtained by photographing

* From the 'Johns Hopkins University Circular' for May, 1889. Communicated by the Author.

the coincidence with the lower wave-lengths, a method which gives them nearly equal weight with those of the visible spectrum.

The full set of observations will be published hereafter; but the present series of standards can be relied on for relative wave-lengths to $\cdot 02$ division of Ångström in most cases, though it is possible some of them may be out more than this amount, especially in the extreme red.

As to the absolute wave-length, no further change will be necessary, provided spectroscopists can agree to use that of my table, as has been done by many of them.

By the method of coincidences with the concave grating the wave-lengths have been interwoven with each other throughout the whole table, so that no single figure could be changed without affecting many others in entirely different portions of the spectrum. The principal difference from the preliminary table is in the reduction to the new absolute wave-length, by which the wave-lengths are about 1 in 80,000 larger than the preliminary table. I hope this difference will not be felt by those who have used the old table, because measurements to less than $\frac{1}{10}$ division of Ångström are rare, the position of the lines of many metals being unknown to a whole division of Ångström. As the new map of the spectrum has been made according to this new table, I see no further reason for changing the table in the future.

No attempt has been made to reduce the figures to a vacuum, as the index of refraction of air is imperfectly known; but this should be done where numerical relations of time-period are desired.

In the column giving the weight, the primary standards are marked S and the other numbers give the number of separate determinations of the wave-length, and thus, to some extent, the weight.

Many of these standards are double lines, and some of them have faint components near them, which makes the accuracy of setting less. This is especially the case when this component is an atmospheric line whose intensity changes with the altitude of the sun. The principal doubles are marked with *d*; but the examination has not been completed yet, especially at the red end of the spectrum.

Standard Wave-Lengths in Ordinary Air at 20° C. and
760 mm. Pressure.

Weight.	Wave-length.	Weight	Wave-length.	Weight.	Wave-length.
1	3094.735	2	3652.689	3	4062.602
1	3095.003	1	3658.679	4	4073.915
1	3109.165	2	3667.398	3	4083.752 <i>d</i>
1	3115.158	2	3683.192	3	4083.925
1	3121.259	2	3684.249	3	4088.711
2	3129.861	2	3695.189	3	4103.096
1	3137.437	2	3707.176	5	4107.636
2	3140.859	2	3716.573	5	4114.593
8	3153.865	2	3732.539	3	4121.474
8	3167.290	2	3747.094 <i>d</i>	3	4121.962
8	3176.095	2	3754.652 <i>d</i>	5	4157.936
3	3188.157	2	3756.204	6	4185.054
2	3200.024	2	3770.122	6	4199.254
3	3218.376	3	3780.843	8	4215.655 <i>d</i>
1	3224.366	3	3781.328	8	4222.374
2	3232.403	3	3783.675	4	4254.490
2	3246.127	3	3794.021	5	4267.941 <i>d</i>
1	3260.390	3	3804.151	5	4293.245 <i>d G</i>
1	3267.841	3	3805.493	4	4308.023 <i>d</i>
1	3287.795	3	3821.316	5	4318.828
2	3292.172	3	3823.648	5	4325.982 <i>t</i>
2	3295.951	3	3836.230	5	4343.380
2	3303.652 <i>d</i>	3	3843.399	5	4352.910
2	3308.928 <i>d</i>	2	3864.449	4	4359.772 <i>t</i>
2	3318.162	6	3875.235	5	4369.946
1	3331.711	5	3883.579	8	4376.102
2	3348.001 <i>d</i>	5	3883.782	6	4391.152 <i>d</i>
2	3351.864	5	3897.605	6	4407.848 <i>d</i>
8	3356.214	5	3916.880	5	4447.899
2	3377.656 <i>d</i>	5	3924.672	4	4494.725
8	3389.879	5	3925.349	8	4497.050
8	3406.566	5	3925.796	3	4499.062
2	3406.941	5	3926.128 <i>d</i>	3	4499.312
2	3425.711	4	3937.472	8	4501.439
2	3455.382	8	3941.038	8	4508.449
2	3464.608	8	3942.555 <i>d</i>	2	4563.937
3	3478.001	8	3950.103	4	4571.274
2	3486.044	8	3950.503	4	4572.157
1	3491.469	7	3954.001	4	4578.723
1	3500.709	4	3960.429	4	4588.386
8	3510.982	7	3971.481	4	4590.124
8	3518.485	6	3977.891	4	4602.173
8	3540.266	3	3981.907	3	4611.437 <i>d or t</i>
1	3545.332	3	3984.067 <i>d</i>	2	4629.504
1	3550.011	3	3986.893	3	4637.675
1	3564.683	3	3987.207 <i>t</i>	3	4638.211
1	3583.484	3	4003.907	5	4643.641
1	3597.190	4	4005.308 7 lines.	3	4668.302 <i>d</i>
2	3612.213	2	4016.569	3	4679.020
2	3623.329	2	4029.791	3	4683.733
2	3623.602 <i>d</i>	3	4048.878 <i>d</i>	8	4686.395
2	3640.533	4	4055.693	8	4690.323

Table (continued).

Weight.	Wave-length.	Weight.	Wave-length.	Weight.	Wave-length.
S	4691.588 <i>d</i>	8	5159.239	7	5415.409
3	4703.173	13	5162.452	7	5424.274
3	4703.976	9	5165.590	7	5434.726
3	4727.625 <i>d</i>	7	5167.580 <i>d b</i> ₄	7	5447.116 <i>d</i> ?
3	4754.222	8	5169.159 <i>d b</i> ₃	8	5455.749 <i>d</i>
3	4805.251 <i>d</i>	9	5171.784	4	5462.725
4	4823.685	8	5172.867 <i>b</i> ₂	7	5463.159
4	4824.312	9	5173.912	8	5463.481
S	4859.929	10	5183.798 <i>b</i> ₁	8	5466.596
S	4861.492	8	5188.947 <i>d</i>	7	5477.113
4	4890.939	8	5193.142	3	5487.961
3	4900.091	8	5198.887	7	5497.722
3	4900.298	9	5202.487 <i>d</i>	4	5501.676
S	4903.485 <i>d</i> ?	8	5204.710 <i>d</i>	5	5506.989
3	4919.177	10	5210.559	5	5513.194
4	4920.685	7	5215.354	6	5528.625
S	4924.102	8	5217.560	5	5535.064
S	4924.950	8	5225.692	5	5543.421
4	4934.243	5	5230.012	6	5544.151
4	4973.257	8	5233.123	6	5555.105
4	4978.779 <i>d</i> ?	9	5242.665	5	5569.843
4	4980.357	10	5250.389	4	5576.313
4	4981.903	10	5250.819	6	5582.192
4	4994.310	10	5253.641	6	5588.976
3	4999.681	10	5261.885	6	5603.099 <i>t</i>
4	5005.895	5	5266.727	6	5615.523
4	5006.296	8	5269.720 <i>E</i> ₂	6	5615.877
5	5007.423 <i>d</i> ?	8	5270.497 <i>d E</i> ₁	S	5624.250
4	5014.411	10	5273.342	S	5624.768 *
5	5020.202	10	5273.559 <i>t</i>	4	5634.164
4	5036.104	10	5276.210	6	5641.661
6	5050.006	10	5281.972	6	5645.834
S	5060.250	8	5283.805	6	5655.707
S	5064.830	11	5288.712	7	5658.094
S	5068.944	10	5296.876	6	5662.746
5	5083.527	8	5300.918	5	5675.651
5	5090.964	9	5307.547	5	5679.249
6	5097.168 <i>d</i>	9	5316.877 <i>d</i> 1474	6	5682.866
7	5105.723	7	5324.372	5	5688.434
8	5109.824	8	5333.094	5	5701.772
8	5110.572	8	5353.587	4	5709.628
6	5115.560	6	5361.810	5	5709.765
6	5121.798 <i>d</i>	4	5363.019 <i>d</i>	6	5715.315
6	5126.371	6	5367.666	7	5731.977
6	5127.534	6	5370.162	6	5742.065
8	5133.874	6	5371.677 <i>d</i>	6	5752.259
8	5139.542 <i>d</i>	6	5379.769	6	5753.343
7	5141.915	8	5383.567	5	5754.886
7	5143.041 <i>d</i>	6	5389.675	6	5763.219 <i>d</i>
8	5146.670	6	5393.366	4	5772.365
8	5151.029	S	5397.342	6	5775.309
7	5154.237	S	5405.977	6	5782.349 <i>d</i> ?
7	5155.940	3	5409.991	6	5784.086

* Peirce's standard.

Table (continued).

Weight.	Wave-length.	Weight.	Wave-length.	Weight.	Wave-length.
10	5788.142	6	6162.395 <i>d</i> ?	6	6518.594
12	5791.211	4	6169.774	5	6532.560
4	5798.094	4	6173.553	7	6534.166
5	5798.404	4	6177.025	7	6546.478
4	5805.451	4	6180.414	4	6552.830
3	5806.957	5	6191.419	8	6563.042 C
8	5809.437	6	6191.770	8	6564.424
8	5816.593 <i>d</i>	6	6200.533	8	6569.450
3	5831.837	6	6213.648	4	6572.307
8	5853.905	6	6219.494	3	6574.473
8	5857.675	6	6230.944	6	6575.170 <i>d</i>
8	5859.812	4	6237.525	9	6593.149
8	5862.582	8	6246.532	10	6594.099
7	5884.053 <i>d</i>	5	6252.780	5	6609.335
4	5889.875	5	6254.454 <i>d</i>	4	6633.974
8	5890.188 D ₂	4	6256.573	8	6643.866
8	5893.103	5	6261.315	2	6663.500
8	5896.156 D ₁	7	6265.348	4	6663.681
5	5898.401	6	6270.440	8	6678.223
8	5901.686	7	6278.297 <i>d</i>	6	6703.799
9	5905.900	5	6281.366	7	6705.343
8	5914.390 <i>d</i>	3	6289.614	7	6717.922
9	5916.480	4	6293.153	7	6722.085
7	5919.862	4	6296.148	5	6726.913
9	5930.415	4	6301.720	8	6750.407
8	5934.889	4	6314.871	5	6752.951
9	5948.755	3	6315.533 <i>d</i> ?	7	6768.038
7	5956.932	8	6318.240	6	6772.564
7	5975.583	8	6322.911	5	6787.129
8	5977.012	8	6335.553	3	6807.100
3	5985.050	8	6337.044	6	6810.518
5	5987.293	4	6344.372	3	6820.609
8	6003.250	6	6355.262 <i>d</i> ?	6	6828.848
5	6008.782	6	6358.907 <i>d</i> ?	4	6841.595
3	6013.724	4	6380.957	3	6843.908
5	6016.858	7	6393.823	7	6855.431
4	6020.351 <i>d</i>	2	6400.207 <i>d</i>	6	6867.462 *
4	6022.021	3	6400.521	6	6867.796
5	6024.273	7	6408.238	7	6870.174 <i>d</i>
4	6027.265	8	6411.868	7	6875.831
6	6042.323	6	6420.178	5	6876.954
5	6056.233	8	6421.573	5	6877.881
8	6065.709	8	6431.065	9	6879.295
8	6078.711	8	6439.301	9	6880.172
7	6079.224	4	6450.028	10	6884.082 †
5	6102.940	7	6462.840 <i>d</i>	9	6886.009
4	6103.429 <i>d</i>	4	6471.889	9	6886.984
4	6108.333	3	6480.282	5	6896.290
4	6111.281	6	6482.104	6	6897.194
4	6116.414 <i>d</i> ?	7	6493.998	6	6901.108
7	6122.432	7	6495.204	6	6909.672
4	6136.830	7	6499.870	6	6919.246
5	6141.931	4	6516.312	5	6923.562

* B 1st line.

† B single line.

Table (continued).

Weight.	Wave-length.	Weight.	Wave-length.	Weight.	Wave-length.
7	6924·422	4	7168·216	4	7318·807
5	6929·823	5	7176·365	3	7321·052
7	6947·777	5	7184·791	3	7331·204
8	6956·708	5	7186·570	2	7335·631
7	6959·715	4	7200·768	5	7446·017
8	6961·521	4	7201·487	5	7495·347
4	6978·657	3	7216·819	5	7511·272
8	6986·841	4	7223·927	4	7594·061
5	6989·254	3	7227·773	8	7621·268
6	6999·171	3	7232·512	5	7623·529
4	7006·166	3	7233·170	5	7624·852
5	7011·585	4	7240·964	4	7627·255
6	7016·701	4	7243·898	4	7628·528
8	7023·762	2	7247·467	3	7659·643
8	7027·727	3	7264·851	3	7660·763
7	7035·167	3	7265·832	4	7665·254
5	7038·479	2	7270·210	4	7666·236
8	7040·069	4	7273·246	4	7670·974
4	7090·686	3	7287·690	4	7671·977
4	7122·522	4	7290·707	2	7699·344
4	7147·958	4	7300·062	2	7714·657
4	7148·438	4	7304·468		

LVI. *On the Visibility of Faint Interference-Bands.* By
Lord RAYLEIGH, *Sec. R.S., Professor of Natural Philo-*
*sophy in the Royal Institution**.

IN a recent paper on the limit to interference when light is radiated from moving molecules †, it was necessary to form an estimate of the ratio of illuminations (h) at the darkest and brightest parts of a system of bands corresponding to the moment when they just cease to be visible from lack of contrast. In the comparison of uniformly illuminated surfaces, brought well into juxtaposition, h might be as great as $\cdot 99 \frac{1}{2}$; but in the case of bands, where the transition is gradual, a higher degree of contrast between the brightest and darkest parts may be expected to be necessary. In order to allow for this, I supposed that h might be estimated at $\cdot 95$, the intensity of the light and the angular magnitude of the bands being assumed to be suitable. But since widely different estimates have been put forward by others, I have thought it

* Communicated by the Author.

† Phil. Mag. April 1889.

‡ See Helmholtz' 'Physiological Optics,' § 21.

worth while to test the matter with bands that are well under control.

In the first experiments light polarized by a Nicol fell upon a slit, against which was held a somewhat stout selenite. Direct examination of the slit through an analysing Nicol revealed no colour on account of the thickness of the selenite; but when a dispersing-prism was added, the resulting spectrum was marked out into bands, whose brightness and contrast depended upon the relative orientations of the Nicols and of the selenite. The theory of these bands is well known*. If the Nicols be parallel, and if the principal sections of the Nicols and the selenite be inclined at the angle α , the expression for the brightness is

$$1 - \sin^2 2\alpha \sin^2 \frac{1}{2}\rho,$$

where ρ denotes the difference of retardations of the two rays to whose interference the bands are due. At the brightest place $\rho=0$, and at the darkest $\sin^2 \frac{1}{2}\rho=1$, so that

$$h=1 - \sin^2 2\alpha.$$

The bands are thus invisible when $\alpha=0$, and increase gradually in distinctness with α . When $\alpha=45^\circ$, the darkest place is absolutely black†.

The selenite was mounted upon a divided circle, and the observation consisted in finding the two positions, on either side of $\alpha=0$, at which the bands manifested themselves with the desired degree of distinctness. The angular interval between the two positions was then taken as representing the value of 2α . In order that the bands should be recognizable with certainty it was found that 2α must be at least 14° . For a distinct and continuous impression $2\alpha=17^\circ$. Corresponding to these, we have for $1-h$,

$$\sin^2 14^\circ = \cdot 0585, \quad \sin^2 17^\circ = \cdot 0855.$$

In these observations the earliest recognition of the bands was somewhat interfered with by a want of smoothness in the spectrum due to irregularities in the selenite. Any irregularity, whether of this kind or caused by dust upon the edges of the slit, gives rise to horizontal markings in the spectrum which distract the eye. In a second set of experiments this

* See, for example, *Enc. Brit.* "Wave Theory," § 22.

† This presupposes an infinitely narrow slit. In practice the width must be reduced until, in this position, the bands are sensibly black.

difficulty was obviated by the substitution for the selenite of an accurately worked plate of quartz, cut parallel to the axis.

The following were the readings by myself (R) and by my assistant (G), when the bands were but just recognizable with certainty.

82° 6'	72° 15'	81° 2'	71° 59'
81 0	71 40	80 43	72 16
82 2	72 40	81 7	72 9
81 41	72 0		
80 31			
Mean...81 28	72 8	80 57	72 8

Hence

$$(R) \quad 2\alpha = 9^\circ 20', \quad (G) \quad 2\alpha = 8^\circ 49';$$

so that, since $\sin^2 9^\circ = .0245$, the bands are visible when $1-h$ is less than half as great as before. The following were the readings when the bands were considered to be still *distinct* :—

83° 15'	70° 33'	82° 58'	70° 52'
83 28	69 53	82 41	71 2
83 0	70 30	83 37	71 20
Mean...83 14	70 19	83 5	71 5

Hence

$$(R) \quad 2\alpha = 12^\circ 55', \quad (G) \quad 2\alpha = 12^\circ 0'.$$

Here $\sin^2 12^\circ 30' = .0372$; so that a difference of 4 per cent. between the darkest and brightest parts is sufficient to show the bands with distinctness.

It seems therefore that I was well within the mark in assuming that bands involving 5 per cent. of the brightness might still be visible.

LVII. *On the probable Cause of the Displacement of Shore-lines, an Attempt at a Geological Chronology.* By A. BLYTT.

[Plate X.]

[Concluded from p. 429.]

THE Trias period has received its name because it shows a distinct triple division. It commences with freshwater and littoral formations, upon which follow formations of deeper water, and then closes with freshwater and shore-formations. At its commencement the land was high relatively to the sea; as it went on the sea rose higher and higher; then the land again began to rise, and at the end of the period it was again high in relation to the sea. And these great changes in the situation of the coast-line were no doubt effected by means of many smaller oscillations.

But just as it is with the Trias, so is it also with other geological formations. They commence with littoral formations (there is often a conglomerate at the bottom); these are followed by deeper marine formations, and at the close we have again shore-formations. The name of Trias would therefore really apply to all of them. The first person to call attention to this remarkable triple division of formations would seem to have been Eaton. It was subsequently discussed by J. S. Newberry in his memoir entitled "Circles of Deposition in American Sedimentary Rocks" (Proc. Amer. Assoc. 1873, vol. xxii. p. 185), and Hull (Trans. Geol. Soc. Glasgow, 1868, iii. pt. 1, p. 39); see also A. Geikie, 'Text-book of Geology,' p. 498, where further references to literature will be found. Principal Dawson called these tripartite periods "cycles," and in his 'Story of the Earth and Man' he established the following cycles of this kind:—1. Cambrian; 2. Lower Silurian; 3. Upper Silurian; 4. Devonian; 5. Carboniferous; 6. Permian; 7. Trias; 8. Lower Jurassic; 9. Middle Jurassic; 10. Upper Jurassic; 11. Cretaceous; and 12. Tertiary*. It appears therefore that these cycles are periods of long duration; each of them has certainly lasted several hundred thousand years. And in the middle of each cycle the great overflows of the sea have attained their highest point. The cycles alternate with continental periods. During the elevation of the land the horizontal position of the strata was often disturbed, so that the deposits of the new cycle lie unconformably upon the older ones.

In this way the development has gone on, at any rate in the northern hemisphere. Mojsisowics, Suess, and others

* We shall see hereafter that this formation includes two cycles.

have pointed out that it has taken place simultaneously in the same direction in Europe, Asia, and North America. These great changes have taken place over the whole of the northern hemisphere, and on both sides of the oceans they have constantly had the same direction. And the same geologists have justly insisted that this law is one of the most remarkable results of geological investigations.

The development of organic life, as we now know, has gone on uninterruptedly from the earliest times. There has certainly never been any general destruction, never any completely new creation. The new has developed from the old through transitional forms and in the course of millions of years. If we knew all the deposits which have been formed it would be impossible to draw any boundaries between geological formations. One would imperceptibly pass over into the other. The boundaries between formations correspond with great gaps in the series of beds. In the time which intervened between the youngest bed in an older and the oldest in a younger cycle, the land in the northern hemisphere lay so high that no marine deposits were formed in the parts of the earth's crust which are accessible to our investigations. Nevertheless the development of living forms went on its even course. But when, after a long time, the land was again submerged, the life in the sea had changed, and beds with new fossils were deposited upon the old ones. And it is from the animal remains of marine deposits that the formations are determined. Hence the sudden change of fossils where a new formation commences is not due to any catastrophe, but simply to a shorter or longer interruption in the formation of deposits in the parts of the earth which we are able to examine. There is no doubt that there are transition-beds between formations, but they lie concealed from us at the bottom of the sea. It is only in certain strongly plicated chains that these beds are upheaved and can be examined. Thus in the Alps there are transitional beds between the Cretaceous and Tertiary, between the Permian and Trias, &c.*

* "As long ago as 1846, Darwin, in his observations in South America, showed that certain assemblages of fossils presented a blending of characters which are of Jurassic and Cretaceous age respectively. Since that date, the study of the fossil faunas of South Africa, India, Australia, New Zealand, and the Western Territories of North America has furnished an abundance of facts of the same kind, showing that no classification of geological periods can possibly be of world-wide application" (J. W. Judd, Presidential Address to the Geological Society, 1888, and 'Nature,' March 1, 1888, p. 426). See also Mojsisowics, *Die Dolomitriffe Südtirols und Venetiens*, Vienna, 1879, p. 36; and von Hauer, *Die Geologie*, Vienna, 1875, p. 515.

If we should attempt to establish geological formations by the aid of the known remains of terrestrial animals and plants, the boundaries of these would not coincide with those which are defined by marine animals*. Thus, to mention an example, the appearance of Dicotyledons does not coincide with the boundary of any formation; but they first appear with a number of forms in the Upper Cretaceous period (Cenomanian).

From Tertiary times, with the exception of the deposits in the great mountain-chains, we know only formations of shallow seas. The Tertiary deposits which correspond to the deep-sea strata of older formations, and which were deposited further from the land during that period without their formation being interfered with by the numerous minor oscillations of the coast-lines, still remain for the most part concealed from us in the sea.

Land-formations, freshwater and littoral formations such as we have in abundance in our Tertiary basins, are greatly exposed to destruction, for they are more frequently elevated above the protecting sea. In the older cycles such formations are more rare, probably to a great extent because they have been destroyed by denudation. We may therefore conclude that the Tertiary formations would much more resemble those of the older cycles if our knowledge of them all were equal. Of the older cycles we often know especially the deep-water formations, of the youngest chiefly those of more shallow waters. At some far distant period the exposed Tertiary formations will come to equal those which are now visible from older cycles.

Dawson (*l. c.* pp. 176-179) expresses the notion that the remarkable regularity with which such cycles recur may perhaps have a cosmical cause and be conditioned by one or another astronomical period. But he seems afterwards to reject this idea, because the Palæozoic cycles have deposits which are four or five times as thick as the Mesozoic (*l. c.* p. 195), and we might therefore believe that more time must have been occupied in their formation. But, on the other hand, he notes that in Palæozoic times changes in the organic world went on much more slowly in relation to the formation of deposits than subsequently, so that the fossils extend through greater thicknesses of strata than in the thinner, newer cycles. If I were to judge from these facts adduced by Dawson, I should

* "The growth of our knowledge concerning the terrestrial faunas and floras of ancient geological periods has constantly forced upon the minds of many geologists the necessity of a duplicate classification of geological periods, based on the study of marine and terrestrial organisms respectively." (J. W. Judd, *loc. cit.* p. 427.)

come to a different conclusion; I should regard it as a probable supposition that the formation of deposits went on more rapidly in Palæozoic times than later on. If the moon at that time were nearer to us and the sidereal day shorter, as Darwin thinks, the tidal wave must both have been stronger and have acted more frequently than at present. The coasts would be destroyed much more rapidly, and the sea would have much more material to deposit. A cycle of this period would be thicker than the younger cycles, and the fossils would extend through a greater thickness of strata than in the latter. For I see at present no probable ground for the supposition that the development of new species would be accelerated in the same degree as the formation of deposits.

There is therefore reason to assume that it is owing to these great changes in the form of the earth, occurring at long intervals, that we can distinguish between geological formations. But such great changes in the distribution of land and sea must necessarily also bring with them considerable changes of climate, and at the same time also changes of living forms. I have already, in one of my memoirs, put forward the opinion that the glacial period had its origin in a change of the distribution of land and sea. If the land gained a great extension in the middle and higher latitudes, especially if there should be a formation of bridges across the sea such as the supposed bridge through the Faröes and Iceland from Scotland to Greenland, the warm sea-currents would be excluded from the higher latitudes. The northern seas would then become icy seas, and where the snowfall is sufficient inland ice would be formed. In a memoir entitled "*Natürliche Warmwasserheizung als Princip der klimatischen Zustände der geologischen Formationen*" (in *Abhandl. Senckenb. Gesellsch.* vol. xiii. p. 277 *et seqq.*), J. Probst (like Sartorius von Waltershausen, in his *Untersuchungen über die Klimate der Gegenwart und Vergangenheit*, 1865) has with justice pointed out the great importance which warm sea-currents have, and have had, in rendering milder the climate of high latitudes.

It has been generally accepted among geologists that during the older formations animal- and plant-life was more uniform over the whole earth than at present. But this opinion must be changed according to recent investigations. Thus J. W. Judd says ('*Nature*,' March 1, 1888, pp. 424 *et seqq.*), with regard to the oldest fossiliferous deposits (the Cambrian):—"Even at that early period there were life-provinces with a distribution of organisms in space quite analogous to that which exists at the present day." Examples of geographical provinces are indicated by him in the Silurian, Trias, Jura,

and Cretaceous; and he says further:—"I believe that the study of fossils from remote parts of the earth's surface has abundantly substantiated Prof. Huxley's suggestion that geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present."

Most deposits of ancient times belong to periods in which the land lay low in relation to the sea, and the difference between the geographical provinces is far less in the great depths of the sea than near the shores and on the solid ground. It has also hitherto been a general theory that the climate in old times was warmer and more uniform over the whole earth than now. The further we go back, it is said, the warmer it was, and this has been regarded as connected with the interior heat of the earth. The Glacial period was an interruption of the continuity of its gradual cooling. In periods of overflow, when the land lay low and the sea had great extension under high latitudes, warm marine currents had much easier access to the Poles than during continental periods. As we now know most about the deposits formed during periods of overflow, and as most of the deposits of continental periods are either removed by denudation or concealed under the sea, it is still probable that the deposits of older cycles might show less strongly marked geographical provinces, and, as a rule, bear witness to warmer climates even under high latitudes. But the great changes in the distribution of land and sea compel us to assume that, hand in hand with them, occurred a periodical alteration of climate, which has been far greater and more radical than the change produced by the precessional periods.

Ramsay, Croll, J. Geikie, and others have thought that they found more or less certain traces of Glacial periods in the older formations (see, *e. g.*, J. Geikie, 'The Great Ice Age,' ed. 2, 1887, pp. 566 *et seqq.*). Some of these traces seem to prove that, at any rate, there have been more Glacial periods than the Post-tertiary one. Nevertheless von Richthofen remarks (*Führer für Forschungsreisende*, p. 362) that these supposed traces of Glacial periods are perhaps only a phenomenon of abrasion, and that the action of the waves upon the shore could produce conglomerates with striated stones. As regards these supposed old Glacial periods, the most certain traces (see J. Geikie, *l. c.*) appear to be furnished by the Devonian Sandstone, "Old Red," in England and Scotland, by the commencement of the Carboniferous period (Scotland), by the Permian conglomerate (England), and by the Eocene (Switzerland). The most striking evidence (with striated stones) is from the periods when the land had great extension.

As regards these great overflows, it must be remembered that it is only in folded chains and in strongly elevated regions (*e. g.* in the Alps, Himalaya, Colorado, &c.) that sea-formed deposits of the later and latest geological periods occur at very considerable elevations above the sea. These great elevations, if we consider them in relation to the whole, can only be regarded as quite local phenomena. At the time when the deposits were formed they lay much lower, and when we now find an alternation of marine and freshwater deposits in such formations we must not suppose that the sea rose and sank in relation to the land by thousands of feet at each oscillation. During the period of formation the shore-line need only have moved up and down a few metres. Afterwards the whole system of strata was lifted high above its original level by locally acting "geotectonic" forces.

Therefore I assume that even the great overflows do not depend upon any very considerable displacement of coast-lines in a vertical direction. When there are large flat countries with basin-shaped depressions, a small elevation may suffice to produce great geographical changes.

Possibly also these overflows may be due to changes in the eccentricity of the orbit.

We will now test our hypothesis by a comparison between the astronomical periods and the geological series of deposits.

The curve of the eccentricity of the earth's orbit has been calculated from Leverrier's formulæ by J. Croll ('Climate and Time,' 1875, p. 312) for a period of four millions of years; three millions of years backward, and 1 million forward from the present time. The curve is also calculated according to the same formulæ by McFarland (*Amer. Journ. Sci.* [3] vol. xx. 1880, p. 105). His calculation extends from 3,250,000 years backward to 1,250,000 years forward in time. He has calculated with shorter intervals of time than Croll (Croll 50,000, McFarland 10,000 years), which, however, has had no particular influence in altering the form of the curves. McFarland has in the same place calculated the curve for the same period of time from new formulæ of Stockwell's. The two curves, taken in the gross, show a uniform course throughout their length, but as regards the first half Leverrier's curve is thrown somewhat backward. Stockwell's formulæ are considered to be more accurate than Leverrier's.

Both curves are given by McFarland. If we compare them together it appears:—

1. The curves coincide with only a small essential difference from the present day until 1 million years back.

2. If we omit the portion between 7' and 8' of Leverrier's curve, Leverrier's and Stockwell's curves are in all essential points identical also as regards the older part, although the agreement is not so complete as for the last million of years. The reason of this is that the calculations are less certain with regard to the older periods ; when the number of years enters as a factor in the formula, small errors in the values adopted for the planets' masses will be enlarged in proportion to the time, and the result becomes less certain.

3. A very remarkable consequence proceeds from these calculations. *The curve repeats itself after the lapse of 1,450,000 years*, when it is calculated according to Stockwell's formulæ. In the period of $4\frac{1}{2}$ million years for which McFarland has calculated it, it repeats itself in this way with remarkable regularity a little more than three times. In each of these cycles there are 16 arcs of the curve. Thus the arcs which in the accompanying plate (Pl. X.) are indicated by 1-16 correspond with 1'-16' and 1''-16''. From calculations which he made at my request, Mr. Geelmuyden has declared that the course of the curve will probably be sufficiently correct to be adopted with safety as the foundation for geological considerations, and that uncertainties in the curve caused by errors in the masses employed by Stockwell will probably not be of any importance.

4. The mean value of the eccentricity is least at the limits of two cycles ; it rises in the first and sinks in the last half of each cycle, and therefore attains its greatest value about the middle of each cycle. Thus for the first and second of the calculated cycles and their subdivisions it is as follows:—

Cycle I.	÷ 3,250,000—2,720,000 years, 0·0304.
	÷ 2,720,000—2,150,000 years, 0·0332.
	÷ 2,150,000—1,810,000 years, 0·0203.
Cycle II.	÷ 1,810,000—1,250,000 years, 0·0247.
	÷ 1,250,000— 700,000 years, 0·0340.
	÷ 700,000— 350,000 years, 0·0280.

Cycle III. ÷ 350,000 to the present time, 0·0291.

Now as, according to our hypothesis, the sea-level under high latitudes will rise and fall with the eccentricity, then it must not only rise and fall once for each arc of the curve, but the “mean sea-level” for longer periods must also rise and fall with the mean value of the eccentricity, and such cycles as cycles I. and II. must then correspond to two cycles in the geological sequence of deposits. The limits between the cycles of the curve must correspond to the periods of denudation which divide the geological cycles, and the middle must correspond to the periods of overflow.

The correctness of the two hypotheses put forward in my memoir on the Alternation of Strata may, as already indicated, be tested in one way by the comparison of geological profiles with the curves of the eccentricity of the earth's orbit. A first attempt was made at the time with the Upper Eocene and Oligocene beds of the Paris-basin.

Many difficulties, however, stood in the way of this work. First and foremost the calculation of the curve is less certain for distant periods. This difficulty is to a certain extent got rid of by the circumstance that, as the curves repeat themselves, it may be less essential.

Another difficulty is in the finding of long and accurately described profiles without gaps in the series of deposits. Survey-profiles are not sufficient. Geologists often only state that there are few, some, or many alternations of strata, without giving definite numbers.

A third difficulty is the distinguishing between the alternations of deposits which are due to precessions and those which have their cause in other more transitory and local conditions. In the case of shore-formations this difficulty is especially perceptible; but it has proved to be less than I supposed at first.

A fourth difficulty consists in the determination of the number of oscillations of coast-lines. The higher a place was situated, the more rarely was it overflowed; the lower it lay, the more rarely was it uplifted above the sea. And movements of the solid body of the earth, as might be supposed, have not been so uniform everywhere as those of the sea.

A fifth difficulty lies in the finding of perfectly typical profiles of the stages produced by the oscillations. When the sea rose and sank slowly the number of marine alternations of strata will be less, and of land and freshwater formations greater, the higher the place lay and the shorter the time which it remained submerged in the sea during each oscillation. But this difficulty is of importance only when the continuous profiles are so short that they do not embrace several oscillations.

In the absence of longer, connected and accurately traced profiles I have first endeavoured to determine the number of oscillations of coast-lines as regards the Tertiary and Quaternary periods. Each of these oscillations, of which there have been about 36 from the commencement of the Tertiary period until now, has, in temporarily submerged localities, produced an alternation of marine beds with freshwater or terrestrial formations. To each more considerable oscillation corresponds a geological "stage." In these "stages" there

is a certain number of alternations. By studying the literature of the Tertiary basins of Europe I have in this way formed a combined profile, which, as regards the alternations of strata, is not yet completed throughout, but which goes from the commencement of the Tertiary to the present time, and which I shall now proceed to describe.

The mode in which profiles can be compared with the curve to test the correctness of the hypotheses is as follows:— Each arc in the curve will correspond to an oscillation of the sea. It is supposed that under high latitudes the coast-lines move up and down with the curve. Such an oscillation I call a “geological stage.” Each arc will therefore have its corresponding oscillation or “stage,” and in each “stage” there will be as many alternations of strata as there are precessional periods in the corresponding arc. When the eccentricity only sinks inconsiderably between two or more arcs, the arcs run into one another and form, as it were, ranges with two or three small summits. We have then “stages” with more oscillations and more alternations of strata than the ordinary ones. We shall see examples of this in what follows. We can draw a line which indicates the boundary between marine and freshwater formations. This line may be nearly or quite horizontal. Whether it is to be drawn high or low depends upon how much above the sea the place was situated where the deposits were formed at the time when the deposition took place. The higher it lay, the higher must the line be drawn. The place may have been so elevated that it never was submerged. Then the lines are situated higher than the curve, and all the deposits are freshwater or terrestrial formations. But it may have lain so low that it never rose above the sea, and all the deposits are marine formations. But the line may cut the curve. Then marine formations alternate with land- and freshwater-formations. The former correspond to those arcs of the curve which project above the line; the latter to those which lie below it. And when there are no gaps in the series of deposits, there will be as many alternations of deposits in the marine, freshwater, and terrestrial formations as there are precessional periods in the corresponding arcs of the curve.

As a starting-point I will take the profile of the Paris-basin*, which I will endeavour to join on to recent times. Afterwards I will refer to the lower and middle parts of the Eocene period.

The section of the Paris-basin about Méry-sur-Oise (*Bull.*

* This section is given in my memoir on Alternations of Strata.

Soc. Géol. Fr. 1878, pp. 243 *et seqq.*) shows the following oscillations and alternations of strata, and may, as regards the continuous portion, fit into Stockwell's curve, as appears from the arc-numbers cited for each oscillation :—

Sables de Cuise, marine.

Calcaire grossier inférieur et moyen, marine, with 7 alternations.

Calcaire grossier ; Caillasses à *Cerithium*, 2 marine alternations, and between them a deposit with freshwater shells.

Calcaire grossier ; Caillasses à *Lucina*, marine, with 5 alternations.

Calcaire grossier : Caillasses à *Cardium*, marine, with 11 alternations. Gap in the series.

Sables de Beauchamp, freshwater and marine, about 4 alternations. Arcs 14–15.

Calcaire de St. Ouen, freshwater, 4, above which a marine deposit (summit of arc 16), then 6 freshwater alternations. Arcs 15–2'.

Gypsum, marine, about 11 alternations. Arcs 2'–4'.

Gypse palustre, freshwater, about 6 alternations. Arc 5'.

Marne verte, brackish, 2 alternations. Arc 6'.

Calcaire de Brie, freshwater, 1 alt. Between arcs 6' and 7'.

Marne et Mollasse, sables de Fontenaye, marine, 3 alternations. Arc 7'.

Meulnières de Montmorency, Calcaire de Beauce (p. p.), freshwater. Between arcs 7' and 8'.

There is only one discrepancy :—Arc 16, the summit of which should correspond to the marine deposit in the middle of the Calcaire de St. Ouen, does not go so high that we should expect an inundation of the sea. But the oscillation is at any rate also indicated in Stockwell's curve, and the marine formation consists of a single bed, and is so faintly marked that it has only recently been recognized.

Another profile from another place in the Paris-basin (la Frette, *Bull. Soc. Géol. Fr.* 1876, pp. 471 *et seqq.*) has the same number of alternations as the above and extends from 13 to 2'. The marine bed at 16 is wanting in this profile, otherwise the same oscillations are indicated.

The profile at Méry-sur-Oise has in all 71 alternations of strata, of which 25 are in the Calcaire grossier. A great part of the calculated curve is therefore filled up by the occurrence of 37 alternations without a gap in the series of deposits. With arc 7' the marine formations of the basin

terminate. In Miocene times came the volcanic outbursts in the Auvergne.

These oscillations of the coast-lines were not confined to the Paris-basin. The sequence of deposits in the basin of the Gironde, which seems to have been connected with the Paris-basin only through the Atlantic Ocean, is as follows (according to Vasseur, *Ann. Sci. Géol.* vol. xiii. pp. 398 *et seqq.*) :—

The Tertiary formations commence with the Middle Eocene : Nummulitic sand and coarse limestone, marine. After this, elevation and erosion. Then again followed a depression : clay with *Ostrea cucullaris* (arcs 14–15), and another elevation : lacustrine limestone of Plassac, and simultaneously with this brackish-water limestone of Bégadan (16–1'). Then a new depression :—marine limestone of St. Estèphe and limestones and marls with *Anomia girondica* (2'–4'). Elevation and erosion :—Mollasse (freshwater) of Fronsadais (6'?). Depression :—Calcaire à Astéries de Bourg (marine, 7'). Elevation :—lacustrine limestone of l'Agenais, level 1 (between 7' and 8'). This is contemporaneous with the Calcaire de Beauce of the Paris-basin. In the basin of the Gironde fresh oscillations took place, namely the following, which are Miocene :—Faluns de Bazas, marine, 8'; elevation : lacustrine limestone of l'Agenais, level 2 (between 8' and 9') ; depression : Faluns de Léognan et Merignac, marine (9'). The so-called Mollasse of Anjou, which is wanting in the basin of the Gironde, is, according to Tournouer (*Ann. Sci. Géol. l.c.* p. 62) younger than 9', but older than the Faluns de Salles of the Gironde ; both are marine, and probably indicate two oscillations, 10', 11'. Then followed another Miocene oscillation, which has left its traces in the basin of the Loire, in the marine Faluns of la Dixmerie (arc 12').

Thus the Miocene period in France had five oscillations. I have not, however, been able to obtain detailed profiles of all these series of deposits.

We now pass to England. In the Memoirs of the Geological Survey of Great Britain, 1856, we have accurate profiles of the Tertiary formations of the Isle of Wight (by Forbes and Bristow). The series of beds, from below upwards, has the following oscillations and alternations* :—

Plastic Clay (brackish?), 4 alternations.

London Clay, marine, at least 11 alternations.

Lower Bagshot, (in part?) freshwater, 7 alternations.

Middle Bagshot (Bracklesham and Barton), the first freshwater, the second marine, and with 5 alternations.

Upper Bagshot, without alternations.

* See Postscript, p. 513.

This part of the series is in part older than the Calcaire grossier, and there are at least one, probably two gaps in it. The following series, on the contrary, is continuous :—

Lower Headon, freshwater and brackish, 7 to 8 alternations (arc 13 and the first part of 14).

Middle Headon, marine, 1 alternation, at 14.

Upper Headon, freshwater and brackish, 5 alternations, between arcs 14 and 15.

Osborne, freshwater, 3 alternations, between arcs 15 and 1'.

Bembridge limestone, freshwater, 3 alternations, between arcs 1' and 2'.

Bembridge oyster-bed, marine, at least 1 alternation, at 2'.

Bembridge marl, freshwater, 6 alternations, arcs 2' and 3'.

Hempstead marl, freshwater and brackish, 2 alternations, 4' or 5'?

Hempstead Corbula-beds, marine, imperfect above by denudation, 1 alternation.

The profiles of the different stages are taken at different parts of the island which have lain at different levels. Bearing this in mind, the series may be fitted into the curve, and at any rate correspond with them pretty closely.

The number of alternations in this last continuous part of this series of deposits is about the same as in the contemporaneous deposits of the Paris-basin, although the beds are more than three times as thick (48 metres in the Paris-basin, 156 metres in the Isle of Wight).

With the marine deposits of Hempstead the marine formations of England are interrupted, and it is only in the Pliocene that we have indications of a new marine submergence. The basalts and volcanic eruptions of Ireland and the Hebrides are probably, at any rate in part, Miocene. Basaltic dykes extend in places across the whole of England ; but the chief outbreaks were on the western side, and hence they can be traced through the Faröes to Iceland.

We will now see whether we can fill up the curve from 7', where the continuous profile from the Paris-basin closes, up to recent times. The uppermost bed of the Paris-basin lies upon the boundary between Oligocene and Miocene. As we have already seen, the Miocene period in France had five oscillations. In Transylvania (according to Koch, in the *Földtani Közlöny*) there are five Miocene stages, namely :—Koroder beds, Kettösmezö beds, Hidasalmas beds, Mezöseger beds, and Feleker beds. All these stages are marine. Even

if they are not throughout separated by freshwater formations, as in the case of several, at any rate, of the French Faluns, they may nevertheless be regarded as corresponding to five oscillations. In the deeper seas the bottom will not always be upheaved above the sea under low eccentricities; but the oscillations will nevertheless operate in changing the fauna, and also frequently the constitution of the deposits.

The Miocene deposits of the Vienna-basin are divided into three principal stages,—the first and second Mediterranean, and the Sarmatian. But if we study the detailed profiles more closely, there appear to have been here also five Miocene oscillations. Thus (according to Suess, *Sitzungsb. Wiener Akad.* 1866) the first Mediterranean stage shows the following sequence of strata from below upwards:—

Beds at Molt, with oyster-shells (broken), at the top with lignite, 4 alternations, arc 8'.—Supposed by Suess to be on the same horizon with the Faluns of Bazas.

Beds near Loibersdorf, Gauderndorf, and Eggenburg, marine, probably with 8 alternations, at any rate in part younger than the beds at Molt (arcs 8' ? and 9').

“Schlier” with gypsum, at the top with land-plants.—Suess calls it “ein ersterbendes Meer,” and seems inclined to regard it as a peculiar stage. Alternations, but scarcely more than two. The last part of arc 9'.

Beds at Grund, marine, with few (3–4) alternations, to judge from Suess' profiles.—The fauna forms a transition from the first to the second Mediterranean stage, and this deposit at Grund is with reason regarded by several Viennese geologists as representing a distinct stage. Arc 10'.

This was followed by the greatest submergence, the second Mediterranean stage (arc 11'), contemporaneous with the French Faluns de Salles. The sea rose quite up into the inner Alpine Vienna-basin. I have been unable to make out the number of alternations in this stage. I have only seen sections of the littoral formations described.

Finally, the last Miocene oscillation, the Sarmatian stage, arc 12'. In some localities (*e. g.* near Constantinople) this stage commences with freshwater covered by marine formations (see Suess, *Antlitz der Erde*, i. p. 419). According to a profile from Hungary (by Peters in *Sitzungsb. Wiener Akad.* 1861) the stage has 4 alternations.

This stage is followed by the Pliocene Congeria-beds, which in the Vienna basin are represented only by brackish-water formations, according to Fuchs (*Jahrb. k. k. Geol. Reichs.*

1875) with 4 alternations ; arc 13'. And with these the marine formations of the Vienna-basin, Hungary, and Transylvania come to a close. Volcanic outbursts commenced in these countries even in the Oligocene period ; they became very frequent in the Miocene, and during this period the Alps rose to great altitudes.

In the basin of Mayence the marine Oligocene formations (Weinheimer marine sand and Septaria-clay) are followed first by a freshwater formation ; then the Miocene period commenced with a depression. But during volcanic eruptions the basin was upheaved and became more and more freshwater. A continuous formation of beds took place. Over the Cerithium-limestone, the Corbicula-limestone and Littorinella-clay were deposited, in all with 20 or more alternations (according to Lepsius, *Das Mainzerbecken*). All these deposits are Miocene.

We now pass further forward in time. The Pliocene has four oscillations, 13', 14', 15', and 16'. We have already mentioned the Congeria-beds of the Vienna-basin. In England there are three oscillations :—Coralline Crag (14'), Red Crag (15'), and Cromer Clay or Westleton Shingle (16'). Profiles of these are to be found in Quart. Journ. Geol. Soc. Lond. 1871 (by Prestwich). The climate of Europe began to become colder in the Pliocene. Even the oldest deposit in the Pliocene of England contains stones which may have been grooved by ice, and at the close of the Pliocene there were already great glaciers ; the Pliocene was followed by the Glacial epoch. We have seen how, during strong and extensive volcanic action, previously marine basins were during Oligocene, and especially Miocene, times uplifted above the sea not to be depressed afterwards (Paris, Vienna, Hungary, the Mayence-basin, and we may add Switzerland), and we have seen that the Alps were upheaved in Miocene times. The Faröes and Iceland were built up, at any rate in great part, at the same time by basalts and lavas ; perhaps, moreover, the submarine bank which connects Europe with Greenland was uplifted during the last portion of the Miocene period. In the Mediterranean, according to Neumayr (see Suess, *Antlitz der Erde*, i. p. 425) the coast-lines at the close of the Pliocene lay even lower than at the present day. No doubt all these elevations have had much influence upon climate. Changes in the length of the day are dependent upon variations of the eccentricity. Geographical changes follow upon the increase of the day, and climate changes with the distribution of land and sea.

The Coralline Crag in England (according to Prestwich)

has a thickness of only 25 metres, and cannot have many alternations. After this stage was formed the land rose, but was again partially depressed under the sea. During this depression was formed the Red Crag, with the Chillesford Clay. In the Coralline Crag two shore-lines were hollowed out one over the other and the new stage lies now on the old shore-platforms. The Red Crag is thinner than the Coralline Crag and cannot include many alternations.

In Belgium, also, we have two Pliocene stages, which correspond to the two English Crag-stages:—the Scaldisien, étages supérieur et inférieur. To these two oscillations of the North Sea correspond two contemporaneous ones of the Mediterranean. Suess calls them the third and fourth Mediterranean stages. And even in the earliest part of the Pliocene the Mediterranean fauna indicates a somewhat colder climate (Suess, *l. c. i. p. 431*).

Italy possesses thick Pliocene formations. Seguenza describes deposits 500–600 metres in thickness from this period. I have been unable to obtain profiles of these deposits. They are in part conglomerates and shore-formations, like the great Miocene Mollasse of Switzerland, and near the shore thick deposits can be formed in a short time.

The profiles of Roussillon (by Depéret, in *Ann. Sci. Géol.* vol. xvii. 1885) show four alternations in the stage contemporaneous with the Coralline Crag (arc 14'). The overlying stage in Roussillon is a freshwater formation. The land had risen. The freshwater stage has several alternations, probably 6–8, so far as I can see from the profiles given, which, however, are not quite accurately described (arcs 15'–16').

We now turn again to England. The fossils of the Red Crag show a colder climate than that of the Coralline Crag, and the Chillesford beds, which belong to the last portion of the Red Crag, have distinctly arctic shells. The Glacial epoch was advancing. After the Red Crag was formed England again rose and became united by land with the continent. Extinct mammals wandered in its forests, which consisted of existing trees (spruce, pines, &c.) and show a temperate climate, milder than that of the Chillesford beds, and about as at present. “The forest-bed of Cromer” was overlain by marine deposits—Westleton Shingle and Cromer Clay (arc 16'). In the latest terrestrial formation at Cromer Nathorst has found Arctic plants (*Salix polaris*, &c.), and the Cromer Clay indicates the vicinity of inland ice. With this the Pliocene closes.

As regards the Quaternary oscillations, we will take the

English deposits as described by J. Geikie ('Great Ice Age,' ed. 2, pp. 387 *et seqq.*) as our guide.

The Quaternary period commences with the retrogression of the ice and with a considerable denudation. Then the sea again rose and covered a great part of the east of England. The inland ice again extended itself and formed a bottom-moraine, "the great chalky boulder-clay" (arc 1"). After this glacial period an elevation of the land seems to have followed, and the ice retreated. But a new depression followed (Bridlington Crag) and a new glacial period (purple boulder-clay, arc 2"). A fresh elevation seems to have followed, with a new interglacial period. Then came a new depression, which was very considerable, and which at Moel Tryfaen in Wales, at Macclesfield, and in Ireland has left marine shells at heights of 1000 to 1300 feet above the sea (nearly approaching that at which the old "seter" or beach-lines in Østerdalen, Læsje, &c. occur). Like the preceding depression, this was also followed by a glacial period, the last (Hessle boulder-clay, arc 3"). Finally the land rose and the ice melted. The Postglacial period came with its four peat-beds (the last portion of 3' and 4"). To arc 4" corresponds a small oscillation of the sea immediately before the recent period. In Scania, Gäravallen, a raised beach-formation, rests upon peat; in Gotland, in the British Islands (Carse Clay, &c.), and even in North America, we may trace the same oscillation of the sea; it was no doubt too great to be capable of explanation by local conditions, compression of peat-beds by shifting sand-dunes, &c.

We have already seen that the land (according to Howorth and Suess) in many places under high latitudes rose considerably in the Postglacial period, and that a corresponding depression took place in the warm coral-seas. The last oscillations therefore affected a great part of the earth. From this we may conclude that this was the case also with the oscillations of former times, and that they have their cause in general cosmical conditions. The small oscillation (arc 4') forms an interruption in this great upheaval under high latitudes. A similar interruption of the depression, if our theory be correct, must be exhibited under the tropics; and in reality in the equatorial parts both of America and the Old World, there are numerous evidences of such a small postglacial oscillation in coral-reefs, which have been upraised several metres and are now lying dry (see Suess, *Antlitz der Erde*, ii. pp. 630 *et seqq.*). These coral-reefs may date from the same time when the northern peat-beds were submerged. The sunken peat-beds with the marine deposits formed during the

depression have been again uplifted, and the raised coral-banks have probably again begun to sink (at Bombay there is a sunken forest), but the depression has not yet brought them down beneath the sea.

We may make one or two further observations upon the Glacial period and its formations. Contemporaneous with "the forest-bed of Cromer" (according to Heer) are the lignites of Dürnten in Switzerland. The fossils show this. They have nearly the same plant-remains, and the same extinct animals. The lignites rest upon and are covered by bottom-moraines, and are therefore "interglacial." They have 7 alternations of peat and forest-beds, and may be fitted into the curve between the arcs $15'$ and $1''$. From this the Alps must have had large glaciers even during the time of the Red Crag. And there is no improbability in this if we remember that *Leda arctica* and other Arctic animals were already living on the English coast at this period, and that the Chillesford beds indicate a much colder climate than the subsequent forest-bed of Cromer.

It is instructive to see how each rising of the sea in England during the Quaternary period had as its consequence the increase of the inland ice. This seems to agree with Croll's theory, that glacial periods are a consequence of great eccentricities. But the scanty traces of glacial periods in the older formations, and above all the distribution of glaciers at the present day, show that geographical conditions have the greatest influence. It is only when these are favourable that a high eccentricity can cause the glaciers to increase; if they are very favourable, there may be a glacial period even during a small eccentricity, as in Greenland at the present day. When the eccentricity increases, the precipitation during rainy periods also increases. If the sea is cold, the precipitation will fall as snow, and in this way the glaciers will grow as the eccentricity increases.

North Germany (according to Jentzsch) has also had three glacial periods with corresponding bottom-moraines (and oscillations?); and in the Alps there have been (according to Penck, *Vergletsch. d. deutsch. Alpen*) at least three glacial periods.

We have thus filled up the curve to the present time, and connected the profile of the Paris-basin therewith. We will now trace the oscillations back to the close of the Cretaceous period in order, if possible, to see how many oscillations are included in the Geological period known as the Tertiary.

The Cretaceous period is separated from the Tertiary by a period of denudation, during which the land was high rela-

tively to the sea. The oldest marine formation of the Tertiary period in Europe is considered to be the limestone of Mons, in Belgium. This indicates the first oscillation; but this submergence appears not to have left traces in the other Tertiary basins. The first marine inundation of the Paris-basin during Tertiary times formed the conglomerate of Rilly and Nemours. It was followed by an elevation of the land, and the marine conglomerate was covered by the freshwater limestone of Rilly. This oscillation in the Paris-basin is perhaps represented in Belgium by the so-called "système Heersien," which is at the bottom a purely marine formation, but has remains of land-plants at the top. Then came a new oscillation, and now England also was partially submerged. Here was deposited the marine Thanet Sand, and upon this the Woolwich and Reading Series (=Plastic Clay), the latter partly a brackish and freshwater formation, and which shows that the shore-line had again retreated. In Belgium the "système Landenien" was formed during this oscillation—below purely marine, above brackish. In the Paris-basin there was formed the marine sand of Bracheux, which was followed by a freshwater formation with lignite (the Lignites de Soissonnais). Then followed a new depression, and again an upheaval. This has left no traces in the Paris-basin; but in England the London Clay was formed, and in Belgium the "système Yprésien." The London Clay commences with a shore-formation of shingle or gravel (Oldhaven Beds), and the upper part of the stage shows that the sea again became shallower, in consequence of a new elevation of the land*. The "système Yprésien" in Belgium is divided into two sub-stages:—the older, a clay with Foraminifera; the younger sandy, with numerous fossils, and therefore probably indicating a shallower sea. A new submergence formed, in Belgium, the marine "système Panisélien" (sand), and in the Paris-basin the marine sand of Cuise. With this the Lower Eocene closes. It has therefore, in all probability, 6 oscillations.

The Middle Eocene is represented in France chiefly by the "Calcaire grossier." In this stage there are 5–6 sub-stages, and in several places breaks in the series of deposits. The Middle Eocene is on the whole marine, but with intercalated freshwater beds, and it probably also represents 6 oscillations. In Transylvania it commences (according to Koch in *Földtani Közlöny*, 1883, pp. 118 *et seqq.*) with alternations of clay and marl, upon which follow alternations of gypsum and marl ("lower gypsum horizon," 1st oscillation). Above it, marine

* This stage, as was shown above, contains at least 11 alternations, and therefore probably corresponds with at least two arcs of the curve.

deposits, the *Perforata-beds*:—from below upwards—(α) an oyster-bed, (β) argillaceous marl, (γ) calcareous marl (“lower striata-horizon”), (δ) a shell-bed (“lower perforata-horizon”), (ϵ) clay (“upper striata horizon,” second oscillation?), (ζ) clay with a few hard marly beds and the same fossils as in β , (η) another oyster-bed, (θ) clay with oysters, (ι) calcareous marl (“upper perforata horizon,” third oscillation?); above this the *Ostrea-clay*, a thick clay with oysters and marly beds, and with a sandy calcareous bed in the middle (fourth oscillation). Over this again the *Lower Coarse limestone*, generally in two thick beds (fifth oscillation), covered by a thick bed of clay varied with layers of sand, probably a freshwater formation, and covered by freshwater limestone. Finally, the last (6th) oscillation, the *Upper Gypsum horizon*, gypsum alternating with clay; and above it coarse limestone alternating with gypsum; in other places clay with Foraminifera, marine:—the *Upper Coarse limestone*. I have cited all these details in order to show that these beds, which are all contemporaneous with the “Calcaire grossier” of Paris, seem to indicate six oscillations.

Above the “Calcaire grossier” the Upper Eocene commences with the continuous series of the Paris basin, which has already been described.

The Lower and Middle Eocene therefore appear to include 12 oscillations, six of which pertain to each of the two divisions of the formation. By this the first cycle of the curve is filled up; so that the beginning of the cycle will about fall upon the boundary between Cretaceous and Eocene. In the Paris-basin the Middle Eocene has 25 alternations of strata, and perhaps one or two breaks. Six oscillations about correspond to 25–30 precessional periods.

At the commencement of the cycles the mean value of the eccentricity is low; it rises in the middle of the cycle, and sinks again towards the conclusion. The position of the shore-lines must also depend upon the mean value of the eccentricity. But as it increases very slowly through very long periods, it will be very long before its action is to be seen on the solid earth. The middle of the cycles ought thus to correspond to the overflows of the sea, the beginning and close to the periods of denudation which separate the formations. Breaks in the series of beds may therefore be expected under high latitudes, especially at the limits between the cycles.

The boundary between Cretaceous and Eocene is indicated by what Suess (*Antlitz der Erde*, ii. 7ter Abschn. p. 376) calls a negative phase: the sea had retreated in higher latitudes.

During the Eocene it rose again, and the Eocene sea had a great extension ; we find its formations even in the heart of Upper Asia. The limit between the Eocene and Oligocene is again distinguished by a negative phase. In the latter part of the Oligocene period, and still more during the Miocene, the sea again rose ; between the Miocene and Pliocene it retreated far, and at the beginning of the Quaternary epoch it rose again. Similar great oscillations are also to be traced in North America and in Patagonia. But marine Miocene deposits are wanting in the last-mentioned locality, where the Miocene freshwater beds are associated with great quantities of volcanic products.

At the commencement of the Tertiary period, when the sea had retreated far under high latitudes, the climate of Europe was temperate rather than tropical (see Saporta, *Le Monde des Plantes avant l'apparition de l'homme*, 1879). According as the sea rose, and the Eocene overflow advanced, the climate became warmer ; and at the close of the Eocene period the climate of Southern Europe was hot and dry. The abundant Tertiary flora of the Arctic lands is (according to Saporta and Gardner) rather Eocene than Miocene (as Heer supposed). At the boundary between Eocene and Oligocene the sea retreated, and the Arctic Tertiary flora began to migrate into Europe, supplanting the more southern plants. Then came the Miocene overflow, and with it a rich tropical or subtropical flora. But in proportion as the Miocene sea retreated, the European flora also, little by little, lost in richness and beauty, and the tropical elements became more and more rare. During the Pliocene epoch the sea retreated still further, and the climate became colder and colder until the Glacial period came in. But the last Quaternary overflow has again, after several oscillations, caused the ice to retreat, and our climate has again become temperate. There is thus clearly a relation of dependency between the climate and geographical conditions. Great seas under high latitudes produce warm climates and *vice versâ*.

Now, we have seen that these great geographical changes were in all probability a consequence of the rising and sinking of the mean value of the eccentricity ; and we must therefore believe that these great changes of the climate had a cosmical origin, and occurred at the same time over the whole earth. We still know too little of the geology of tropical countries ; but there is ground for the belief that here also great changes have taken place in the distribution of land and sea, and that these changes must also have had an influence upon the climate of the warm countries.

It is, further, probable that the force of vulcanicity stands in relation to the changes in the eccentricity. Each of the great geological formations, from the Pre-Cambrian itself, has had its volcanoes (see A. Geikie, 'Textbook,' pp. 259-260); and we have already seen that the same author states that there have been periods in the earth's history when vulcanicity was much more powerful and widely distributed than at other times. We have seen how the upheaval of the land was accompanied by volcanic outbursts; and as regards the Tertiary period, at any rate, it appears that the great overflows of the sea were followed by periods during which the solid ground began to rise during violent and wide-spread volcanic eruptions.

For easy reference we will finally enumerate all the arcs in the curve, and name the geological stages supposed to correspond to them. To some extent we adopt the names given by Charles Mayer Eymar*.

LOWER TERTIARY; EOCENE.

Cycle I. in the curve.

LOWER EOCENE. Arcs 1-6.

From 3,250,000 years to 2,720,000 years before the present time.

- | | | |
|--------|-----------------------|--------------|
| Arc 1. | Etage Montien? | |
| 2. | " Heersien. | |
| 3. | " Suessonien. | |
| 4. | " Yprésien inférieur? | } Londinien. |
| 5. | " Yprésien supérieur? | |
| 6. | " Panisélien. | |

MIDDLE EOCENE.

From 2,720,000 to 2,150,000 years before the present time.

Arcs 7-12. Etage Parisien, with 6 oscillations.

UPPER EOCENE.

From 2,150,000 to 1,810,000 years before the present time.

Arcs 13-16. Etage Bartonien, with 4 oscillations.

* See his valuable *Classification des Terrains Tertiaires* (Zurich, 1884). He divides his stages into two substages—one with "mers amples," and one with "mers basses." Some of his stages, however, represent several oscillations. He thinks that the precession of the equinoxes is the cause of the changes in the level of the sea. The whole of the Tertiary and Quaternary periods must, according to him, have had a duration of only a little over 300,000 years. He founds his views upon Schmick's untenable hypothesis of the dependence of the sea-level upon the precessions.

UPPER TERTIARY. Cycle II.

OLIGOCENE.

From 1,810,000 to 1,160,000 years before the present time.

Arcs 1'-4'. Etage Ligurien, with 4 oscillations.
 5'-7'. „ Tongrien, with 5 oscillations.

MIOCENE.

From 1,160,000 to 700,000 years before the present time.

Arc 8'. Etage Aquitanien ?
 9'. „ Langhien.
 10'. „ Helvétien.
 11'. „ Tortonien.
 12'. „ Messinien.

PLIOCENE.

From 700,000 to 350,000 years before the present time.

Arc 13'. Etage Matérin.
 14'. „ Plaisancien.
 15'. „ Astien.
 16'. „ Arnusien.

QUATERNARY. Cycle III.

From 350,000 years ago, to the present time.

Arcs 1''-3''. Etage Saharien, with 3 oscillations.

The limits between the cycles of the curve are not drawn arbitrarily. The beginning and the close of the first two cycles are distinguished by their unusually low eccentricity. The last arc in one cycle and the first in the following one have, together, a duration of about 150,000 years; and in all this time the eccentricity was very low. In these two cycles, likewise, the highest mean eccentricity occurs in the middle of the cycle.

The Eocene period appears to have had 16 oscillations, and should correspond to the first cycle; the Oligocene, Miocene, and Pliocene have likewise together 16 oscillations, and correspond to the second cycle. The Lower Eocene corresponds to arcs 1-6, the Middle Eocene to 7-12, and the Upper Eocene to 13-16. In the same way the Oligocene corresponds to arcs 1'-7', the Miocene to 8'-12', and the Pliocene to 13'-16'. There is thus a certain analogy between the older and the younger Tertiary periods. We have here 6 divisions,

which nearly correspond to each other in the following manner :—

Lower Eocene to the Oligocene : the former with 6, the latter with 7 oscillations.

Middle Eocene to the Miocene : the former with 6, the latter with 5 oscillations.

Upper Eocene to the Pliocene : both with 4 oscillations.

The great overflows of the sea occur in the middle of the cycles, in the Middle Eocene, the Upper Oligocene, and the Miocene. In the middle of the cycles the mean value of the eccentricity was greatest. At the commencement and the last part of the cycles, when the mean value of the eccentricity was small, the sea retreated far, as between the Cretaceous and the Eocene, and in the Upper Eocene and Pliocene. The notion therefore presents itself with great probability that there is a connexion between the cycles in the curve representing the eccentricity of the earth's orbit and what is called a geological epoch, or what has also been called a "cycle" or "circle of deposition." The two Tertiary cycles are as it were great stages, each composed of 16 smaller ones. Just as each of these 16 represents a small oscillation of the sea, so does each cycle represent a great oscillation ; but this great oscillation has been accomplished by means of the 16 small ones. In the same way the mean value of the eccentricity rises and falls in each cycle with 16 oscillations ; it is low at the commencement of the cycle, attains its greatest value in the middle of the cycle, and falls again towards the close. These agreements between the cycles of the curve and the formations, between the arcs of the curve and the stages, and between the number of the arcs' precessions and the alternations of the strata in the stages wherever these could be checked, appear to me to be so striking as to exclude the notion of an accidental coincidence, and distinctly point to a causal relation.

If we would test the correctness of our hypotheses by means of the older formations, the following points must be borne in mind:—After investigating the laws of the variations of eccentricity, Geelmuyden told me that it is probable that a cycle of about 1,500,000 years must appear in the curve, but that without more extended investigation we cannot conclude that this will continue unchanged for unlimited periods. Even in the calculated curve the Cycle III. is distinguished from the other two by a much lower eccentricity in the arcs 4''-9''.

If the polar compression in old times was greater, then the
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precessional period was also shorter. According to Geelmuyden it would be very nearly proportional to the square of the time of rotation. For example, to a rotation-time of 16 hours corresponds a (synodic) precessional period of 10,000 years, consequently only half the present period. The shorter the period the less marked (other things being equal) must the climatic period be, and the more indistinct the alternation of the strata.

Further, it must be remembered, that in Palæozoic and Mesozoic times the moon was probably much nearer. In that case the lunar tide was much stronger, and stronger in proportion to the solar tide than at present. The day was shorter, and the stronger tidal wave acted more frequently. The shores were more rapidly destroyed. Deposition, no doubt, took place more rapidly. The sidereal day increased more quickly in length than at present. All these circumstances must have had an influence upon the form of the earth, upon the distribution of land and sea, upon the displacement of shore-lines, upon the changes of climate, upon the ocean-currents, upon the distribution of chemical and mechanical sediments and the alternations of strata, so that, without taking these and perhaps other circumstances into consideration, we cannot prove the applicability of the hypotheses to the Palæozoic and Mesozoic series.

In conclusion, I will briefly notice the chief points in my hypothesis.

The precession of the equinoxes and the periodical change in the eccentricity of the earth's orbit, are reflected in the series of strata and furnish the key to the calculation of the duration of geological epochs.

Precession causes winter and summer to be alternately longer and shorter. In the semi-period when the winter is longer than the summer, the difference between the inland and coast climate becomes more marked. The atmospheric currents become stronger. As a consequence of this the currents of the ocean increase in strength, and this again reacts on the climate. The periodical change of the climate caused by precession is not very considerable, but still great enough to leave its mark in the alternations of strata, and in the formation of shore-lines, terraces, series of moraines, &c. One alternation of strata corresponds to each precessional period.

The eccentricity of the earth's orbit is periodically variable. Its mean value rises and falls in periods of about $1\frac{1}{2}$ million years, with 16 oscillations. Such a rise and fall I call a

cycle, and each cycle, in the calculated curve, is composed of 16 arcs.

The tidal wave, which is the most important agent in altering the sidereal day, and which makes it longer, rises and falls to a certain extent with the eccentricity. It so predominates over the other forces which alter the length of the sidereal day, that the day steadily lengthens on the average more rapidly in the middle of the cycles when the mean value of the eccentricity is greatest, and more slowly at the boundaries between them, when it is least, and, as regards the individual arcs, with increasing rapidity during rising, and decreasing rapidity during falling eccentricity.

The interior of the earth is plastic in consequence of the great pressure. The surface or "crust," opposes the greatest resistance to change of form. But as the sidereal day lengthens and the equatorial parts of the earth increase in weight, a constantly increasing strain acts outwards towards higher latitudes, and this strain increases until the resistance is overcome. It must also be remembered that forces which are too small to effect any sudden alteration in a solid body, may, nevertheless, produce a change of form when they act for a long time.

Hence the lengthening of the sidereal day does not act only upon the sea, but also upon the form of the solid earth. The earth constantly approaches more and more to the spherical form; but the solid earth, in its movements, lags behind the sea, which accommodates itself at once to the altered time of rotation.

As the motive power of these movements of the sea and the solid earth is periodically variable in accordance with the eccentricity of the orbit, these movements also take place periodically more rapidly and more slowly. And as the sea always adjusts itself to the forces before the solid earth, it is probable that the shore-lines oscillate up and down once for each rising and sinking of the eccentricity of the orbit. This applies both to the individual arcs of the curve and to the cycles. In such a cycle "the mean level of the sea" rises and falls once during 16 oscillations.

According to Darwin the sidereal day has become several hours longer. It is therefore probable that so great a strain must have accumulated in the mass of the earth, that a slight increase of the strain would suffice to effect changes of form at the weakest points. It is also probable that these partial changes in the solid body of the earth must occur especially during great eccentricities, or some time after them, when the motive power increases most rapidly.

The change in the tidal wave with the eccentricity is supposed to be sufficiently great to explain the displacement of shore-lines. A vertical displacement of the shore-line by a few metres is sufficient to produce, in the deeper basins, an alternation of many metres of thick marine and freshwater deposits. And as regards the changes of the solid mass of the earth, we must remember that the series of strata is not complete at any single place. In other words, the oscillations were not general to such an extent as to render them contemporaneous everywhere. It is only by partial changes of form, sometimes here, sometimes there, at those points which were weakest at each period, that the solid earth has approached the spherical form. To each arc of the curve, therefore, there corresponds only a partial, not a general, alteration of the form of the solid earth. And the oscillation of the shore-lines corresponding to the arcs, therefore, cannot be demonstrated everywhere, but only in the basins where the forces at the time exerted their action. Hence we can only obtain a complete profile by combining the beds of all the Tertiary basins. Nor were the changes of the solid earth everywhere equally great, but they were greatest at the most yielding parts of the surface, so that very considerable local upheavals may be consequent upon small changes in the length of the sidereal day. This applies to the individual oscillations; but even the great overflows of the sea of which one falls in each cycle need not be due to any very great rise in the level of the sea, for great plains may be flooded and drained by a comparatively small vertical displacement of the shore-line. But these great changes in the distribution of land and sea were undoubtedly great enough to cause considerable alterations of climate. Great seas in high latitudes render their climate mild, and *vice versâ*.

If now, keeping these principles in view, we compare the curve of the eccentricity with the geological series of strata, we find an agreement which indicates that the hypotheses are correct. The two cycles of the calculated curve correspond to two geological cycles. Each of these cycles has 16 arcs, which correspond to 16 smaller oscillations of the shore-lines, or 16 geological stages. In each of these stages there are as many alternations of strata as there are precessions in the corresponding arc. And the "mean sea-level" rises with the mean eccentricity in the middle of the cycles, and falls at the boundary between them, and hand in hand with the mean sea-level the temperature in the higher latitudes also rises and falls.

The theory here discussed agrees with Lyell's great

principle. Slow changes in the length of winter and summer and in the force of the tidal wave produce periodical changes of climate and displacements of shore-lines. The changes take place so slowly that the effects begin to appear distinctly only after the lapse of many thousands of years. There are two astronomical periods which are the cause of the great and fundamental changes of which geology bears testimony to us from long past days, and which will still continue, for millions of years, to effect similar changes in the geography of our globe, in its climate, and its animal and vegetable life.

POSTSCRIPT.

With reference to the profile of the Isle of Wight above cited (p. 497), I must make a few remarks. Although with some doubt, I have referred the Headon beds to the Upper Eocene. But the difference between the faunas of the Grès de Beauchamp and the Middle Headon is far too great for these beds to be synchronous.

The cause of the error is that I reckoned too many alternations of climate in the Isle of Wight beds. In these fluvio-marine deposits there is by no means the same regularity as in beds which are formed in basins with less sedimentation. The river eroded its borders and shifted its bed, banks were formed and carried away, according as the direction of the stream varied and the channel changed. Hence lenticular intercalations were often formed in the beds, and as precipitous cliffs of the Isle of Wight break down, the minor details of the profiles change in appearance. But with all this irregularity there are certain beds which appear far more constantly, and which we can recognize in the different profiles even although their condition is somewhat altered. By the aid of these constant beds we find order in the variations, and it appears that the great features of the profiles are maintained unaltered; and it is these great features that we must follow when we wish to determine the number of climatic alternations. In the Paris-basin, where sedimentation was much less, chemically deposited beds play a much more prominent part. In the Isle of Wight the stages are of much greater thickness. The Oligocene deposits of the Isle of Wight are 156 metres in thickness, and more than three times as thick as the contemporaneous beds in the Paris-basin, which have a thickness of only 48 metres.

In the dry periods the deposition of clay and mud was much less, the water of the river was purer, and chemically formed

beds had time to be deposited. Instead of clay and marl, limestone, *Septaria*-beds, ironstone, &c., were formed. These beds were undoubtedly formed much more slowly than the sand-, clay-, and marl-deposits which alternate with them. They are analogous to the forest-beds in peat-mosses. Forest-beds often separate peat-deposits with different species of plants. This shows that the forest-beds indicate long dry periods, during which the formation of peat ceased, and the flora became changed *; when the quantity of rain again increased and the formation of peat commenced anew, the forest-trees which grew around the mosses were changed, and the forest-beds thus make divisions between different substages or zones in the peat.

Among the beds deposited in water (whether fresh- or salt-water formations) it is chiefly the above-mentioned chemically-formed beds that are formed in dry periods. And just as in the peat-mosses forest-beds often separate peat-deposits with a different flora, so limestone- and *Septaria*-beds also frequently intervene between clay-, marl-, and sand-deposits with a more or less different fauna, so that these chemically-produced deposits often form boundaries between geological stages and substages. This is the case, for example, in the Fluvio-marine series of the Isle of Wight, the main features of which we shall now pass on to describe with the aid of Forbes's detailed and classical statements. We shall then see that we have fewer climatic changes than I previously supposed, and that the series of beds in the Isle of Wight coincides as admirably with the curve of eccentricities as the Parisian deposits, although somewhat later on in time than was hitherto supposed; thus the agreement with the palæontological results becomes complete.

We begin from below, with the Upper Eocene *Barton Clay*. Judging from the fossils this is synchronous with the Grès de Beauchamp in the Paris-basin. It has 5 *Septaria*-beds, and corresponds to arc 14 of the curve, which has the same number of precessional periods. The Barton Clay is covered by the *Headon Sands* (previously referred to the Upper Bagshot), which have no alternations, and which were probably formed in a comparatively short time.

A great gap now follows in the series in the Isle of Wight. In the Paris-basin the freshwater Calcaire de St. Ouen was formed at this time. This is only 6-7 metres thick, but it has 10 alternations, which should represent 200,000 years according

* See "Theori om Indvandringen af Norges Flora under vekslende regnfulde og tørre Tider," in *Nyt Mag. for Naturv.* xxi. 1876 (pp. 52, 53 of separate copies).

to my calculation. It might seem that this was a long time for the formation of a stage of so little thickness ; but while this stage was deposited the marine fauna was changed to such an extent that a great geological boundary has been drawn through this point, the boundary between the Eocene and Oligocene. The first marine Oligocene bed in the Isle of Wight (the Marine Headon) has a fauna of which only 30–50 per cent. of the species occur in the Barton Beds. The 6–7 metres of the freshwater limestone in the Paris-basin probably represents more than half the time which elapsed between the formation of the Marine Barton and Headon.

Then the sea rose again, and the Oligocene period commenced. The oldest Oligocene stage in the Isle of Wight is the Lower Headon ; it is a fresh- and brackish-water formation showing one oscillation of the shore-line. I have given it 7–8 alternations. The stage contains five limestones, separated by deposits of sand and clay, and besides these, two horizons with ferruginous concretions. Reckoning these, it has 7 periods. Marine fossils (*Cytherea*, *Mytilus*) sometimes occur in the middle of the stage ; freshwater and brackish forms above and below. The Lower Headon thus represents one oscillation of the shore-line (or a little more) with 7 climatic changes.

The next stage or oscillation is the *Middle* and *Upper Headon*. These have together 6 alternations of strata, 4 limestones, and 2 beds with iron concretions separated by clays and sands. The Middle Headon is brackish at the base, but soon becomes a purely marine formation, with an abundant fossil fauna. The Upper Headon contains fresh- and brackish-water animals.

Above the Headon come the *Osborne Beds*, a nearly pure freshwater formation. It has 8–10 alternations:—2 *Septaria*-beds, 2 ironstone bands, and 6 horizons with concretions of argillaceous limestone, separated by clay, and marl. 10 alternations represent 2 oscillations and 2 arcs of the curve.

Over the Osborne comes the *Bembridge Stage*. The Bembridge beds consist of : first, a freshwater limestone, which has 3 well-marked alternations of compact limestone with clay and marl ; these 3 alternations recur in profiles from the most different localities ; over this the marine Bembridge Oyster-bed, and immediately above this a *Septaria*-bed, of which Forbes says that it is “very remarkable and constant.” Above this come the Lower Bembridge Marls with brackish and freshwater animals, but without alternations ; upon this a *Septaria*-bed, “sometimes siliceous, sometimes

calcareous," which forms the boundary between the two substages, the Lower and Upper Bembridge Marls. In these Upper Marls, which likewise contain brackish and freshwater shells and even lignites, I have assumed 4 climatic alternations:—there are two pyritous bands and a marly bed, and at the top, at the limit of the overlying Hamstead* stage, a bed with ferruginous concretions capped with marl. But the two pyritous bands and the first of the above-mentioned marls constitute no palæontological boundary, and are far from being so prominent as the *Septaria*-bed. I therefore regard it as the most probable assumption that the whole of the Bembridge Marls indicate only 3 alternations of climate, and thus for the whole stage we have 6 climatic periods.

Finally, we come to the *Hamstead* Beds*. These at the lowest part consist of brackish and freshwater marls, in which, besides a pyritiferous horizon of little importance, and which forms no palæontological horizon, we find indications of two dry periods. One of these, the so-called "White Band," a more or less hardened ferruginous bed rich in fossils, forms the boundary between the two substages, the Lower and Middle Hamstead Marls; and higher up there is a bed of ironstone concretions, which nearly coincides with the limit between the substages of the Middle and Upper Hamstead Marls. The uppermost part of the Hamstead Stage is formed by the marine *Corbula*-bed, in which there is a bed with *Septaria*. The stage therefore represents one oscillation with 3 climatic alternations; but it is not completely preserved, the top having been removed by denudation.

If we now sum up the above statements, we obtain the following numbers of oscillations of shore-lines and climatic alternations:—

Barton, 1 oscillation, with 5 climatic alternations.

Headon Hill Sand, without alternations.

Break in the series.

Lower Headon, 1 oscillation (or a little more), 7 alternations.

Middle and Upper Headon, 1 oscillation, with 6 alternations.

Osborne, with 8–10 alternations, corresponding to 2 oscillations.

Bembridge, 1 oscillation, with 6 alternations.

Hamstead, 1 (incomplete) oscillation, with 3 alternations.

Besides the Eocene Barton there are 3 well-marked marine

* The name is also often written "Hempstead," but this is incorrect.

Oligocene horizons in this series :—the Middle Headon, Bembridge Oyster-bed, and Hamstead Corbula-beds. The Middle Headon is regarded by palæontologists as synchronous with the marine gypsum in the Paris-basin. I have fitted the Paris beds, so that the marine gypsum coincides with the arc 3', and the Fontainebleau Sands with the arc 7'. If we now arrange the equivalent beds in the Isle of Wight in the same arcs, we see that the Isle of Wight profile fits perfectly into the curve of eccentricity, as follows :—

Lower Headon to the arc 2' and perhaps the last part of 1', with 7 alternations and 7 precessions.

Middle and Upper Headon, with 5 alternations, to the arc 3', with 5 precessions.

Osborne, with 10 alternations, to the arcs 4' and 5', with 10 precessions.

Bembridge, with 6 oscillations, to the arc 6', with 5 or 6 precessions.

Hamstead, with 3 alternations, to the first part of arc 7', with 3 precessions.

It thus appears that the 3 marine horizons coincide with the 3 highest eccentricities, the summits of the arcs 3', 6', and 7', while the lower arcs and parts of arcs correspond to brackish and freshwater beds. The most unmixed freshwater formation, the Osborne, coincides with the two lowest arcs, 4' and 5'.

For the sake of comparison, we will again carefully go through the profile of the Paris basin, and compare this with Stockwell's curve, commencing from the bottom. The beds are numbered in the same way as in the original description of Dollfus and Vasseur (*Bull. Soc. Géol. Fr. sér. 3, tom. vi. 1878, pp. 243, et seqq.*).

Sables de Beauchamp et Mortefontaine &c., beds 89–111.—Arc 14 and first half of 15. In this series we have, first 5 marine sandstones alternating with sand ; then a limestone and a calcareous marl, with intercalated sand and marl. Thus in all 6 or 7 alternations.

Calcaire de St. Ouen, beds 112–142.—A freshwater formation which is divided by a marine deposit (128) into two subdivisions. In the lower part (from the summit of arc 15 to the summit of arc 16) there are 4 horizons of hard limestone and siliceous limestone with intercalated marls. Then comes the marine bed (at the summit of 16). It must be remarked that the corresponding arc in Leverrier's curve reaches higher up. In the upper division of freshwater limestones we have 6 alternations of hard limestone and

siliceous limestone with marl and clay. This division therefore finishes a little to the left of the summit of arc 2'.

Sables de Monceaux, beds 143–145. Marine sand with 3 *Septaria* layers.—The rest of arc 2'.

Marnes à Pholadomya, beds 146–154. Marine, with 2 alternations of siliceous limestone and marl.—The first part of arc 3'.

Gypsum No. 3, beds 155–158. Marine marl and gypsum, 1 alternation, and

Marne à Lucina, bed 159.—The rest of arc 3'.

(The beds 146–159 thus have together 3 alternations and correspond to the arc 3'.)

Gypsum No. 2, beds 160–196, arc 4'.—Marine, at any rate for a great part. But it must be remarked that no fossils have been cited from the last part of this series. Gypsum alternating with marls about 5 times. The most important gypsiferous horizons are the beds 161, 171–176, 178–188, 191 and 194.

Gypsum No. 1, bed 197, 8 metres thick, with freshwater animals. 1 alternation.—Between arcs 4' and 5'.

Marne bleue, beds 198–204, and *Marne blanche*, beds 205–209, freshwater marls alternating with marly limestones and ferruginous marls about 4 or 5 times.—Arc 5' and the first third of arc 6'.

Marne verte, beds 210–217, a brackish-water formation with 2 alternations of clay with marl and siliceous limestone.—The upper part of arc 6'.

Calcaire de Brie, beds 218–220, a freshwater limestone. Perhaps we have here indications of several climatic alternations, for limestones occur alternating with marl 3 or 4 times, though certainly in very thin beds.—Its place is in the hollow between arcs 6' and 7'.

Marne et Mollasse Marine, beds 221–231. Clay alternating with marly limestone and sandstone 3 or 4 times.—The upper part of arc 7'.

Sables de Fontenaye, bed 232. Marine sand with a few layers of clay, but without marked alternations.—The latter part of arc 7'.

Calcaire de Beauce (p. p.).—Freshwater, between arcs 7' and 8'.

From this we get a complete agreement with the palæontological results, as shown by the following comparison of the equivalent formations in both basins :—

PARIS.

Grès de Beauchamp, &c.
 Calcaire de St. Ouen.
 Sables de Monceaux.
 Marne à Pholadomya, Gypse
 No. 3, Marne à Lucina.
 Gypse No. 2-1. Marne bleue.
 Marne blanche.
 Marne verte.
 Calcaire de Brie.
 Marne et Mollasse marine.

ISLE OF WIGHT.

Barton Clay. Headon Hill Sand.
 Wanting.
 Lower Headon.
 Middle and Upper Headon.
 Osborne.
 Bembridge Limestone.
 Bembridge Oyster-bed.
 { Bembridge and
 Hamstead Marls.
 Hamstead Corbula-beds.

It will be seen that the number of alternations of strata is about the same in the synchronous formations in the Paris and Hampshire basins. This shows that this alternation of strata was due to a general cause, and that this cause is the precession of the equinoxes seems highly probable.

As, moreover, the curve of the eccentricity of the earth's orbit appears at the same time to be a curve of the variations of the sea-level, we may also conclude with probability that for one reason or another the sea rose and fell with the eccentricity.

LVIII. *On the Achromatism of Interferences.*

By M. MASCART*.

1. **I**N a communication to the Académie des Sciences, M. Cornu† investigated the phenomenon of the displacement of interference-fringes in white light by the interposition in the path of one of the rays of a refracting plate, and showed that the new position in which the central fringe appears depends not only on the optical retardation, but also on the dispersion of the interposed medium. He applied the name "*achromatic fringe*" to that corresponding to the same phase for the most important colours of the spectrum.

Prof. Stokes‡ had already pointed out this property, and had proved by experiment that observing the fringes through a prism of small angle produces an apparent displacement of the fringe from symmetry.

It should also be added that, since the first application of the method of fringe-displacement to the measurement of indices of refraction, Fresnel§ recognized that it was in-

* Translated from the *Comptes Rendus*, March 25, 1889, p. 591.

† *Comptes Rendus*, xciii. p. 809 (1881).

‡ Brit. Assoc. Rep. pt. 2, p. 20 (1850).

§ *Œuvres de Fresnel*, ii. p. 268.

correct. In one of his experiments on the topaz he only observed a retardation of 16·6 undulations, the calculated value being 21, and he adds :—

“The difference 4·4 is too great to arise from inaccuracy in my micrometric measurements ; but it is possible that the dispersion by the twofold refraction, that is to say, the difference of energy of the twofold refraction for rays of different colours, modified the superposition of the fringes produced by these different rays in such a manner that errors were caused in the determination of the position of the central band, and that it was owing to a like source of error that the discordance in question partly arose.”

2. The employment of a prism produces another effect which is very remarkable : at the same time that the refraction changes the direction in which the fringes of different colours appear it modifies their angular aperture in an unequal manner ; the superposition of systems corresponding to neighbouring wave-lengths is then much more complete than in the original phenomenon, and a considerable number of fringes can be distinguished on either side of that which is achromatized.

It was by observing in this manner through a prism the rings produced by a layer of air between two glasses, that Newton perceived more than forty of them ; the appearances are then analogous to those presented by the employment of a homogeneous light.

To explain this peculiarity I shall consider, in a more general manner, any phenomenon of fringes localized on a sensibly plane surface S , such as ordinary interferences received on a screen, Newton's rings, the bands of chromatic polarization in a crystal of variable thickness, &c. I shall assume, moreover, that the fringes are symmetrical with respect to a straight line to which they are normal, and that the point of observation is in the plane of symmetry, the eye being furnished with a prism of which the principal section is parallel to the same plane.

Taking the axis of x on the right line of symmetry, and the axis of y normal to the surface S , let h be the ordinate and $-a$ the abscissa of the point P , where the eye furnished with the prism is situated, x the abscissa of the point M regarded, i the angle which the ray MP makes with the normal, D the deviation produced by the prism, and θ the angle which the refracted ray makes with the normal.

The difference in path Δ of the rays which interfere at the point M should be considered in general as a function of x and of i ; the deviation D is itself a function of i and of the index of refraction n . Denoting the wave-length by λ , we may then write

$$\left. \begin{aligned} \Delta &= m\lambda = f(x, i), \\ a + x &= h \tan i, \\ D &= \theta - i = \phi(i, n). \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (1)$$

The order m of the *achromatic* fringe is defined by the condition that, for a constant value of m , the angle θ which determines the direction of the emergent rays be the same for the neighbouring colours. If we differentiate these equations, putting $dm=0$, and $d\theta=0$, and replacing the expression $-\lambda \frac{dn}{d\lambda}$, which depends only on the nature of the prism, by L ,

$$\text{we have} \quad m\lambda \left(1 + \frac{\partial \phi}{\partial i}\right) = L \left(\frac{h}{\cos^2 i} \frac{\partial f}{\partial x} + \frac{\partial f}{\partial i} \right) \frac{\partial \phi}{\partial n}. \quad . \quad . \quad (2)$$

The law of the phenomenon of interference being known, as well as the nature of the prism and its direction, equations (1) and (2) determine all the quantities m , x , i , and θ which correspond to the achromatic fringe.

As the angle of divergence of a fringe is in general very small, we shall obtain it by making $dm=1$ in the differential equations and considering λ and n as constants. The angle of divergence $d\theta$ of a fringe is then determined by the equation

$$\lambda \left(1 + \frac{\partial \phi}{\partial i}\right) = \left(\frac{h}{\cos^2 i} \frac{\partial f}{\partial x} + \frac{\partial f}{\partial i} \right) d\theta. \quad . \quad . \quad (3)$$

For a fringe of any order this angle depends on the wave-length, but in the neighbourhood of the achromatic fringe we may take into account equation (2), which gives

$$d\theta = \frac{1}{m} L \frac{\partial \phi}{\partial n}. \quad . \quad . \quad . \quad . \quad (4)$$

The factors L and $\frac{\partial \phi}{\partial n}$ vary very slowly with the colour.

We see then that, whatever be the law of interferences, the value of $d\theta$ in the neighbourhood of the achromatic fringe is in inverse ratio to the order m and is almost independent of the wave-length, so that a very great number of fringes will appear there.

It is even possible, by making choice of a suitable inclination for the prism, to render the coincidence still more perfect, if the differential coefficient of the product $L \frac{\partial \phi}{\partial n}$ with respect to the index of refraction is zero, which is given by the condition

$$L \frac{\partial^2 \phi}{\partial n^2} + \frac{dL}{dn} \frac{\partial \phi}{\partial n} = 0. \quad . \quad . \quad . \quad . \quad (5)$$

Calling A the angle of the prism, β the angle made by the entering-face with the surface S , r the angle of refraction of the ray MP on this face, r' and i' the angles of emergence, we readily find that the equations which determine the order of the achromatic fringe, the angular distances of the neighbouring fringes, and the condition for best achromatism, become

$$\frac{m\lambda}{L} = \left(\frac{h}{\cos^2 i} \frac{\partial f}{\partial x} + \frac{\partial f}{\partial i} \right) \frac{\sin A}{\cos(\beta - i) \cos r'} \quad (2')$$

$$d\theta = \frac{L}{m \cos r \cos i'} \frac{\sin A}{\cos(\beta - i)} \quad (4')$$

$$\frac{1}{L} \frac{dL}{dn} = \sin(\beta - i) \tan r \cos i' - \sin A \tan i' \quad (5')$$

We easily see that the employment of a grating as dispersion-apparatus would not produce an analogous effect.

We will apply these results to two particular cases.

Interference-fringes.—Let us consider first the case of ordinary interferences, where the fringes are equidistant and symmetrical with respect to one of them for all colours. The origin being taken at the centre of the phenomenon, the difference in path Δ is simply proportional to x , and we have

$$m\lambda = \alpha x, \quad \frac{\partial f}{\partial x} = \alpha, \quad \frac{\partial f}{\partial i} = 0.$$

There exists only one achromatic fringe, of which the order m and the abscissa x are given by the equations

$$x = \frac{m\lambda}{\alpha} = h \frac{L}{\cos^2 i} \frac{\sin A}{\cos(\beta - i) \cos r'}.$$

These values of m and of x are the greater, all other things being equal, the farther the prism is from the surface S .

If the observation is made in a direction normal to the surface, that is to say, the screen being discarded, on the path of the interfering rays, there remains simply

$$x = \frac{m\lambda}{\alpha} = hL \frac{\sin A}{\cos \beta \cos r'}.$$

Newton's Rings.—If the rings are produced by a layer of air between a plane surface and a spherical surface of radius R which do not touch, the thickness of the layer at a distance x from the centre may be expressed by

$$e = e_0 + \frac{x^2}{2R} \quad (6)$$

The difference in path at the point considered is then

$$m\lambda = 2e \cos i = \left(2e_0 + \frac{x^2}{R} \right) \cos i,$$

and we have

$$\frac{\partial f}{\partial x} = \frac{2x}{R} \cos i, \quad \frac{\partial f}{\partial i} = -2e \sin i.$$

The condition for achromatism becomes

$$e \left(\frac{\cos i}{L} + \frac{\sin i \sin A}{\cos (\beta - i) \cos r'} \right) = \frac{hx \sin A}{R \cos i \cos (\beta - i) \cos r'}. \quad (7)$$

This curious fact also presents itself, that the final equation which will give one of the unknowns, such as x or θ , is not of the first degree; thus there may exist several distinct achromatic fringes and several groups of visible fringes.

If, for example, we make $i=0$, which corresponds to vision along the normal to the surface, equation (7) reduces to

$$x^2 - 2hL \frac{\sin A}{\cos \beta \cos r'} x + 2Re_0 = 0.$$

The problem is possible only when the condition

$$2Re_0 < h^2 L^2 \frac{\sin^2 A}{\cos^2 \beta \cos^2 r'}$$

is satisfied, and the two corresponding values of x are in this case positive.

One of these values is zero for $e_0=0$. If, therefore, under these conditions, we observe ordinary coloured rings by means of a prism, the central spot remains achromatic, and we perceive at some distance a group of branches of rings, the more compact and the more numerous the higher the order of the achromatized fringe. This is Newton's experiment.

3. If the interference takes place between plane waves, as in Jamin's apparatus, the rings produced by diffusion in thick plates, the phenomena of chromatic polarization in thin plates with parallel faces, &c., we must replace equations (1) by the following:—

$$\left. \begin{aligned} \Delta &= m\lambda = f(i), \\ D &= \theta - i = \phi(i, n), \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (8)$$

which amounts simply to suppressing the variable x .

The order of the achromatic fringe, and the angle of divergence of the neighbouring fringes, are in this case determined by the equations

$$\left. \begin{aligned} \frac{m\lambda}{L} &= \frac{\partial f}{\partial i} \frac{\sin A}{\cos (\beta - i) \cos r'} \\ d\theta &= \frac{L}{m} = \frac{\sin A}{\cos r \cos i'} \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (9)$$

The value of $d\theta$ is the same as in the case of localized fringes. I will take one example only.

Herschel's Fringes.—By placing the hypotenuse-face of an isosceles prism on a thin plate of glass, in such a manner as to leave between the surfaces a layer of air of sensibly constant thickness e , W. Herschel* perceived a series of curvilinear fringes parallel to the curve which defines total reflexion. These fringes show themselves clearly when the rays of light are made parallel: they are therefore produced by the interference of plane waves. In this case the angle β is zero, and the difference in path equals $2e \cos i$. If we denote by $\phi(i, n)$ the deviation $i + i' - A$, which is the same thing as changing the sign of i in the preceding equations, the angle of divergence of a fringe of any order m is

$$di' = \frac{1}{m} \frac{\cos^2 i}{\sin i} \frac{\cos r'}{\cos r \cos i'} = m \frac{\lambda^2}{4e^2} \frac{\cos r'}{\sin i \cos r \cos i'}.$$

As these fringes are very near to total reflexion, we may replace the angles r , r' , and i' by their values R , R' , and I' , corresponding to this finite direction, and we have obviously

$$di' = m \frac{\lambda^2}{4e^2} \frac{\cos R'}{\cos R \cos I'}.$$

Thus Herschel's fringes present this very singular character, that their apparent size, at least in the case of the first ones, is proportional to the order of the fringe and to the square of the ratio of the wave-length to the thickness of the layer of air.

The condition for achromatism is

$$\frac{\cos^2 i}{\sin i} = L \frac{\sin A}{\cos r'}.$$

The distance of the achromatic fringe from the limit of total reflexion is therefore independent of the thickness of the layer. The number of fringes visible in the neighbourhood of this region is greater the greater the thickness of the layer itself.

Talbot† observed this phenomenon in light transmitted through a layer of air compressed between the hypotenuse-faces of two exactly similar prisms, and was able to count as many as 200 fringes with white light.

The fringes which appear at the margins of the polarized field in Nicol's prisms or in Foucault's prism are to be explained in the same manner.

* Phil. Trans. p. 274 (1809).

† Phil. Mag. [3] ix. p. 401 (1836).

LIX. *Notices respecting New Books.*

A Treatise on Hydrodynamics, with numerous Examples. Vol. II.
By A. B. BASSET, M.A. Cambridge: Deighton, Bell, and Co.,
1888.

WE have not had long to wait for the completion of this important work, and now we possess a valuable treatise on Hydrodynamics carefully brought up to date. Considering the great advances made by the recent work of Lord Rayleigh, Poincaré, Hicks, J. J. Thomson, G. H. Darwin, and Love, the author must have devoted much time and trouble to the task of embodying their results and processes in a concise form in this second volume.

So many analytical difficulties of pure mathematics block the way to future progress, that the present work is necessarily only addressed to advanced mathematicians, conversant with Bessel, Elliptic, Spherical Harmonic, Spheroidal, and Toroidal Functions; and incidentally in the course of the hydrodynamical investigations many elegant theorems in Pure Mathematics drop out, important enough to set up in his business a pure mathematician.

Chapter xii. of the present volume is a *resumé* of the chief properties of these functions, required in chaps. xii. and xiv. on the properties of rectilinear and circular vortices, the latter embodying the most important researches of J. J. Thomson.

In chapter xv. the motion of a rotating liquid ellipsoid under the influence of its own attraction is discussed, from the points of view of Jacobi, Riemann, Dirichlet, Sir W. Thomson, Poincaré, and G. H. Darwin. The discussion of the stability of the motion is one of the most refined operations of modern analysis, and important in its bearing on the subject of the Figure of the Earth, and General Cosmogony, including the Genesis of the Moon, recently investigated by Mr. Love, too recently, however, to be embodied in the present volume.

Chapter xvii. treats of all the hitherto solved questions on Liquid Waves, and here analytical difficulties still restrict us to the consideration of waves of small displacement. This restriction is, however, a legitimate working hypothesis when we come to the grand Theory of the Tides, chapter xix., with which the names of Newton, but principally of Laplace are associated; and now recently undertaken afresh by Prof. G. H. Darwin.

The remainder of the volume is devoted to the investigation of the simple cases of liquid motion it is possible to discuss mathematically at present, when the disturbing effect due to a viscosity in the liquid in motion is taken into account. Some important experiments made recently by Prof. Osborne Reynolds have redirected attention to this important question, and shown the path for theoretical investigation.

We venture to defend again the expression "with numerous examples" appearing on the title-page, an expression which will call forth disapproval in certain quarters. These examples are carefully selected and verified; they afford a welcome recreation to

the enthusiastic student; and finally possess the great advantage of condensing into a few lines a mass of information, which otherwise would, in the continental fashion, have swelled this book to double its present dimensions.

The student of Hydrodynamics should be very grateful to the author, Mr. Basset, for an elegant treatise, which will place him abreast of the present state of development of the subject.

A. G. GREENHILL.

LX. *Intelligence and Miscellaneous Articles.*

ON THE PASSAGE OF ELECTRICITY THROUGH BAD CONDUCTORS.

BY HUGO KOLLER.

THE author closes the imperfect dielectric to be investigated with a known resistance in the circuit of a voltaic battery, and determines the fall of potential, which is found to be proportional to the strength of the current.

The transport of electricity through imperfect dielectrics is seen to be different from that through true conductors in three points.

1. If a current of constant electromotive force traverses a condenser formed of an imperfect dielectric, its intensity diminishes with the time at first rapidly, and afterwards more slowly, approaching a definite limit along an asymptotic curve. With one and the same dielectric these changes in intensity take place the more rapidly the greater the tension in the condenser, and the smaller the quantity of dielectric between the electrodes.

2. The strength of the current increases more slowly than the electromotive force producing it.

3. The apparent specific resistance of dielectrics diminishes with their thickness.

This behaviour involves deviations from Ohm's law, of which the former is the more important, as the two latter may perhaps be partially if not altogether referred to the difference in the variation of intensity according to the dielectric tension and the thickness of the condenser.

Following an indication of Maxwell, the author endeavours to explain the decrease in the intensity of the current by referring the apparent currents which pass through a dielectric to three processes which are equivalent to currents.

1. To dielectric displacement.
2. To the formation of a residue.
3. To true conduction.

The influence of the first factor is soon exhausted, while the latter is independent of the time. The author considers the second, the influence of which becomes less with time, and finally disappears, as a transition between dielectric displacement and conduction. The quantities of electricity required for an electrical displacement remain in the form of electrical energy, while the quantities of electricity consumed in keeping up the conduction are transformed into heat. Imperfect conductors are not able to

transport more than one part of the quantities furnished by conduction; a portion remaining in the transitional stage between dielectric displacement and heat, in the form of a sort of packing in the substance of the dielectric. The quantities of electricity stored up in this way in the course of time form the residue. The true resistance of the substance can only be obtained after its complete development. The current which traverses the imperfect dielectric has thus acquired a constant intensity, for the quantity of electricity arriving each moment is usually equal to that converted into heat, and is not partially used in the formation of a residue.

The investigation extended to the following substances:—Petroleum ether, oils of turpentine, of linseed, castor oil, olive oil, almond oil, bisulphide of carbon, oil of vaseline, benzole, toluole, xylene, water, alcohol, ether, and the solid insulators in ordinary use.

Of liquids the best insulators are the products of distillation of petroleum, although individual specimens differ very materially from each other.

The specific resistance of petroleum, ether, vaseline oil, and benzole was in most cases higher than 10^{18} Siemens units.—*Berichte der Akad. zu Wien*, Feb. 1889.

THE ISOTHERMS OF GASES. BY M. AROLDO VIOLI.

The author deduces from the kinetic theory the general relation between pressure, volume, and temperature of a gas. The beginning of the investigation is occupied by an historical account of the efforts hitherto made in this direction which have led to various formulæ. On the basis of various hypotheses as to the mode of action of the molecules, and of the surrounding atmospheres, the following equation is finally established:—

$$\left\{ H + \frac{a}{2\{v(1-b)(1+at)\}^2} \right\} v(1-b) = R.$$

In this H is the pressure in metres of mercury, t the temperature in degrees Centigrade, α the coefficient of expansion of perfect gases, v the volume reduced to 0° , a the constant of molecular attraction, b the ratio of the volume of the molecules to the volume of the gas, and R is a constant. Separate expressions are propounded for the magnitudes a and b ; namely

$$a = 0.000004568 p^2 \sqrt[3]{nh} - \frac{1}{13596h},$$

$$b = 0.0005 \sqrt{ph};$$

in which p is the molecular weight for $H_2=2$, h the pressure in metres of mercury, and n the number of atoms in a molecule.

These formulæ have been applied to gases in various directions, and the results compared with the theories of Van der Waals and of Blaserna. The conclusion consists of investigations on molecular velocity as well as on the critical point.—*Rend. della R. Acc. dei Lincei*, vol. iv. pp. 285 et seqq.; *Beiblätter der Physik*, vol. xiii. p. 66.

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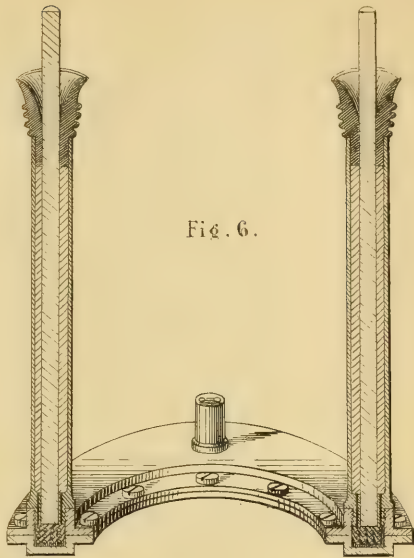


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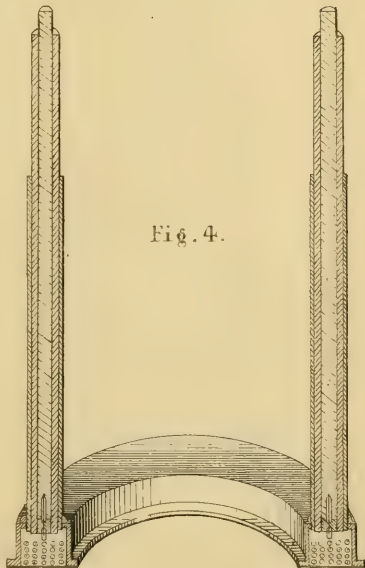
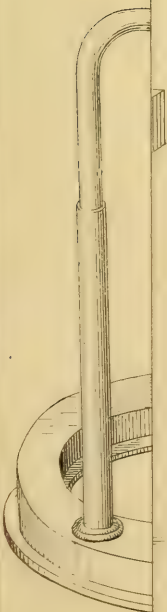


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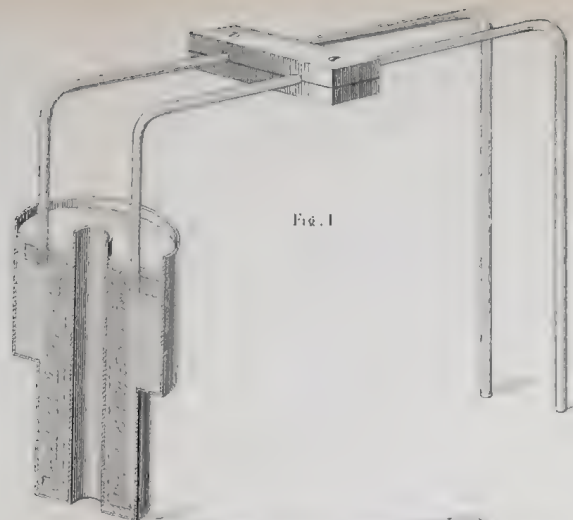


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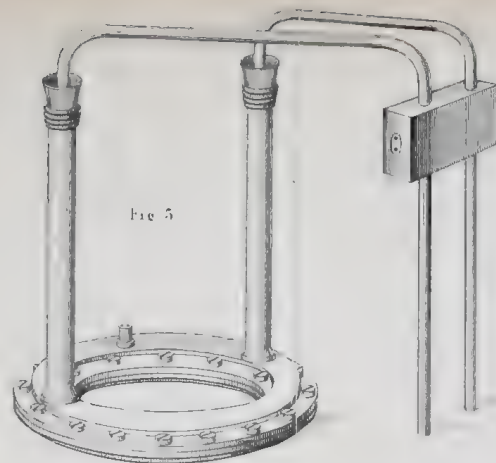


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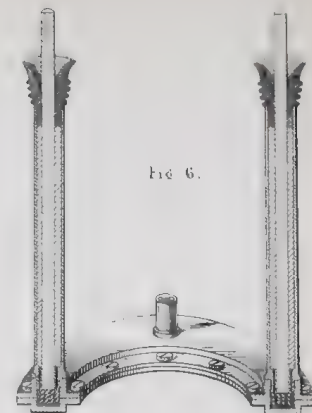


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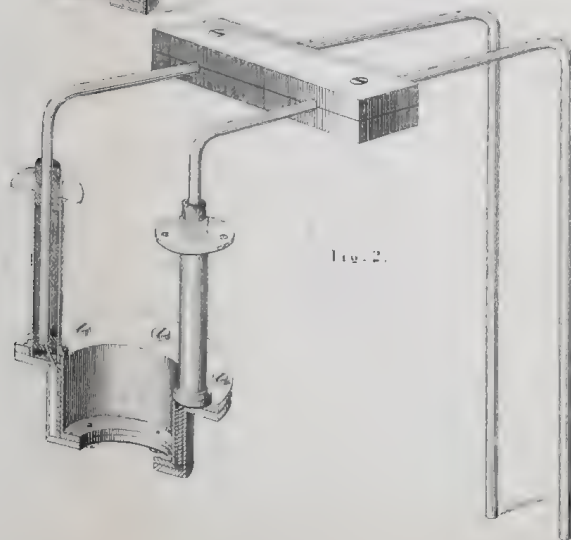


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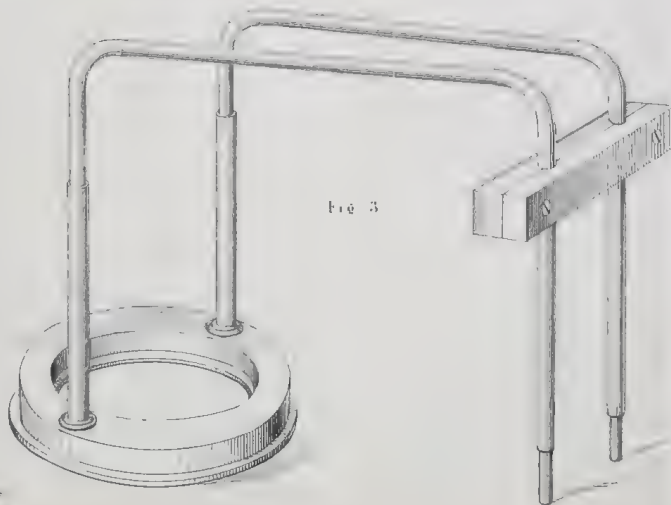


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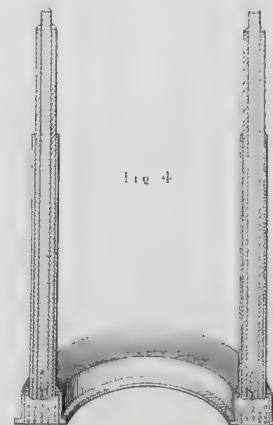
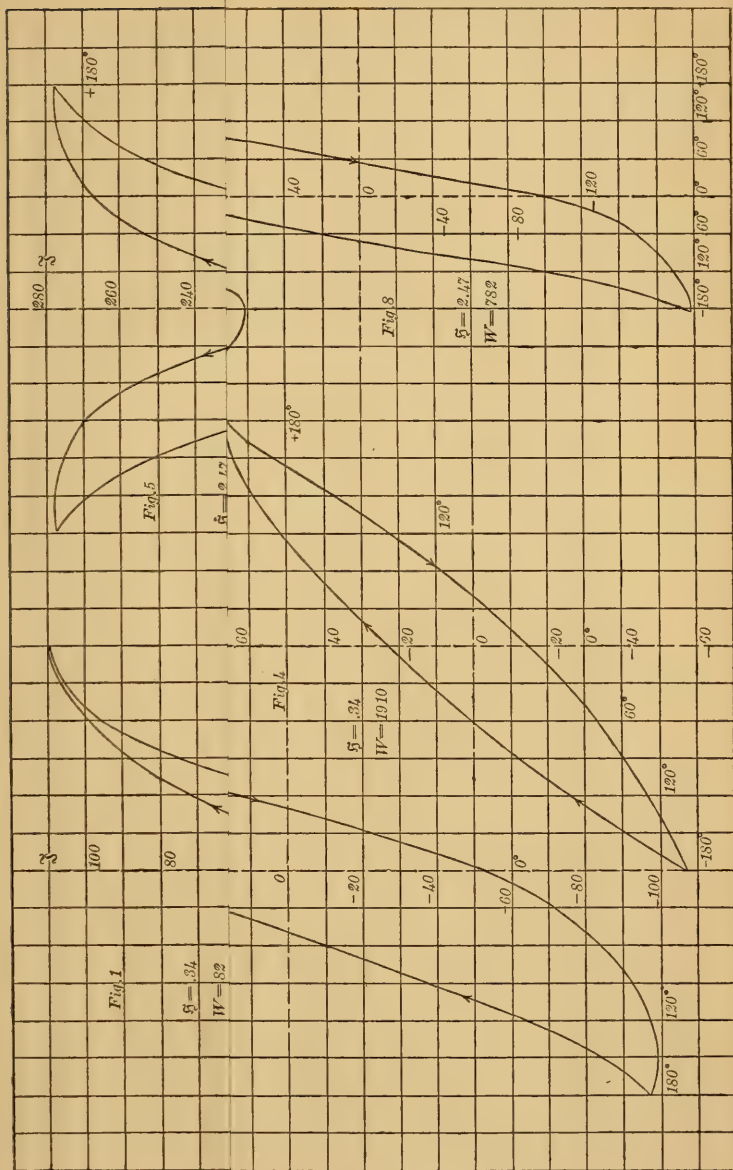
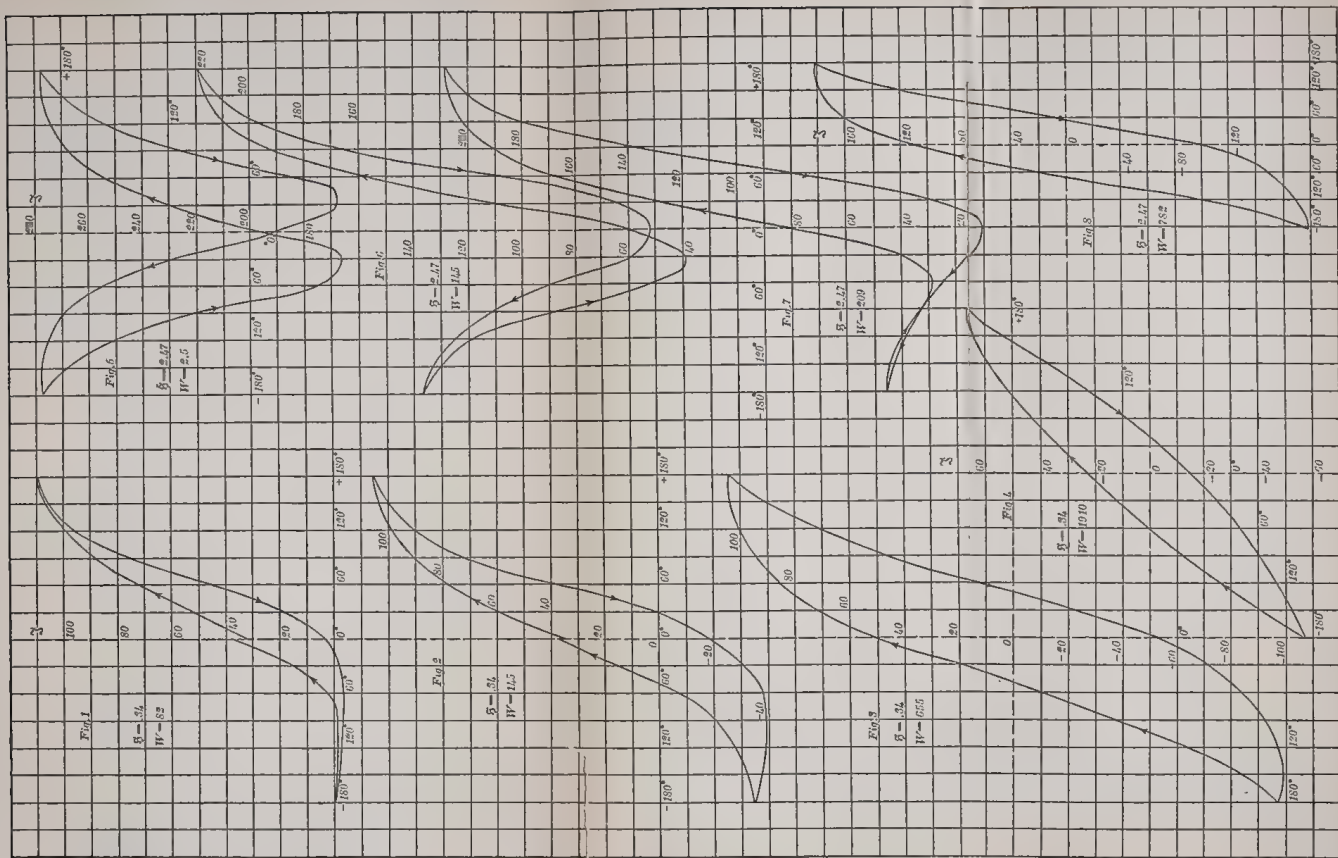
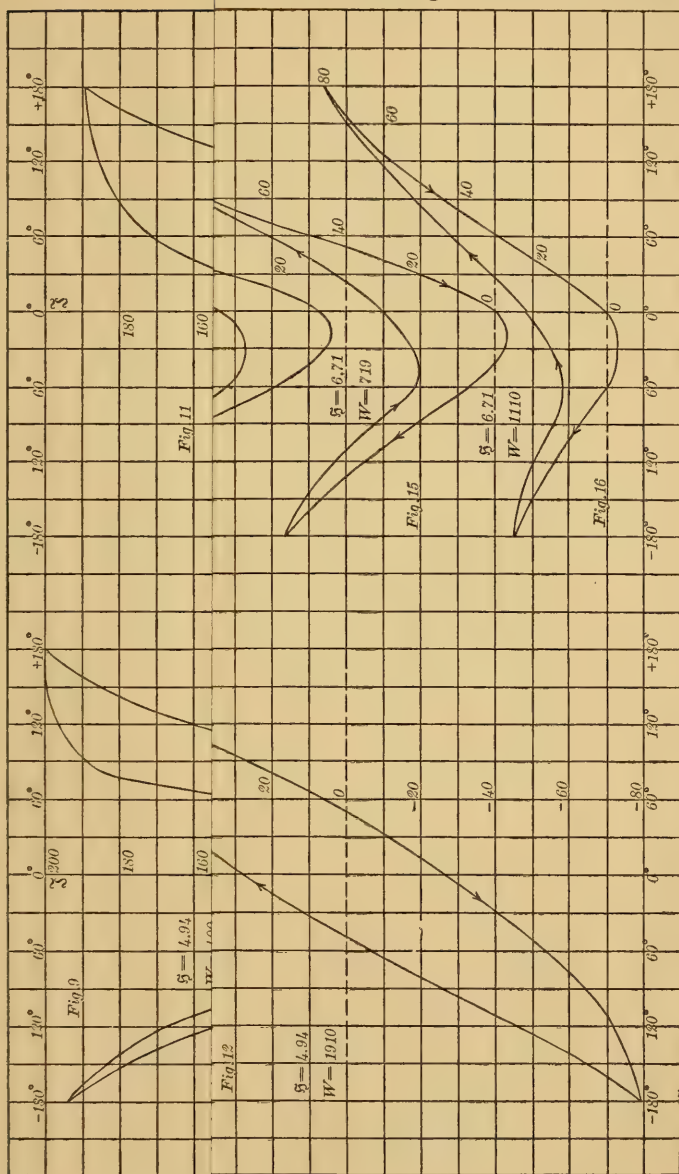
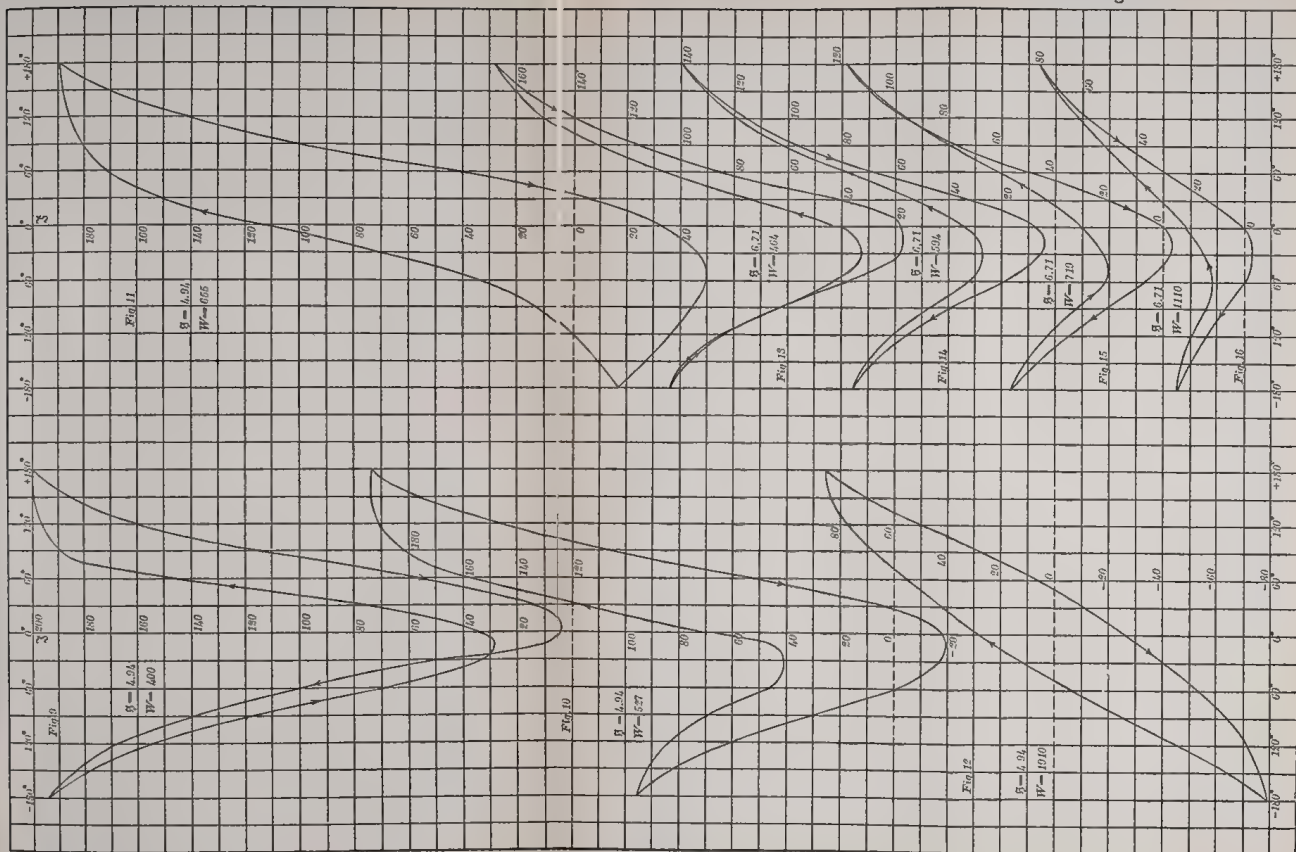


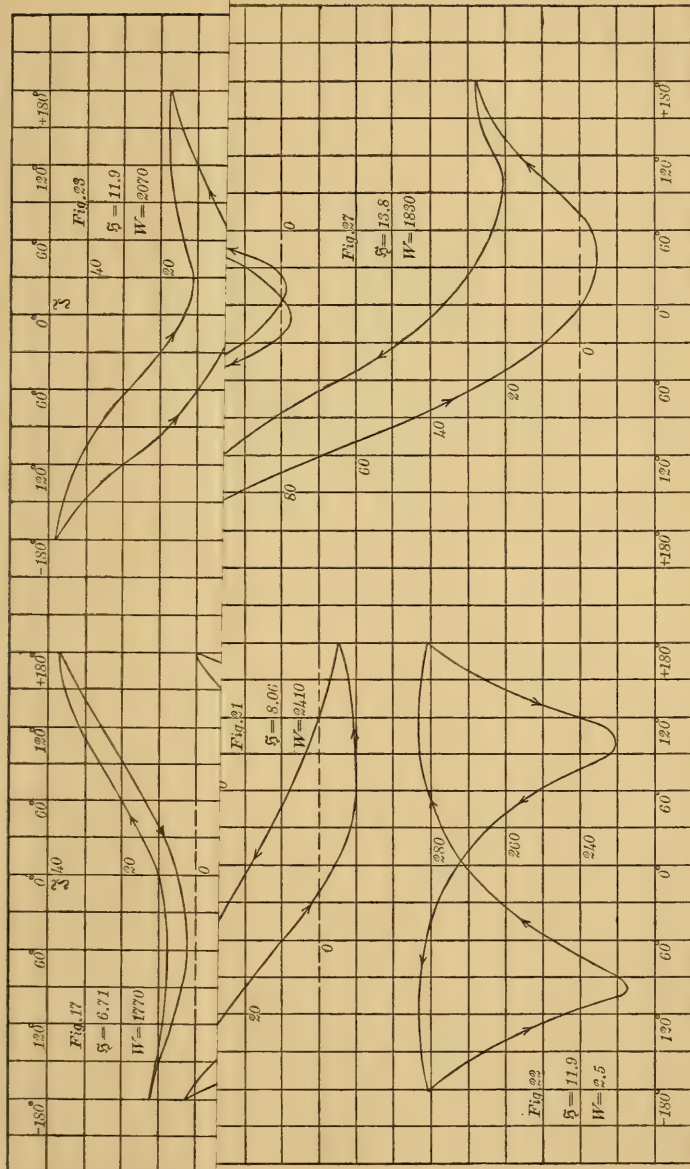
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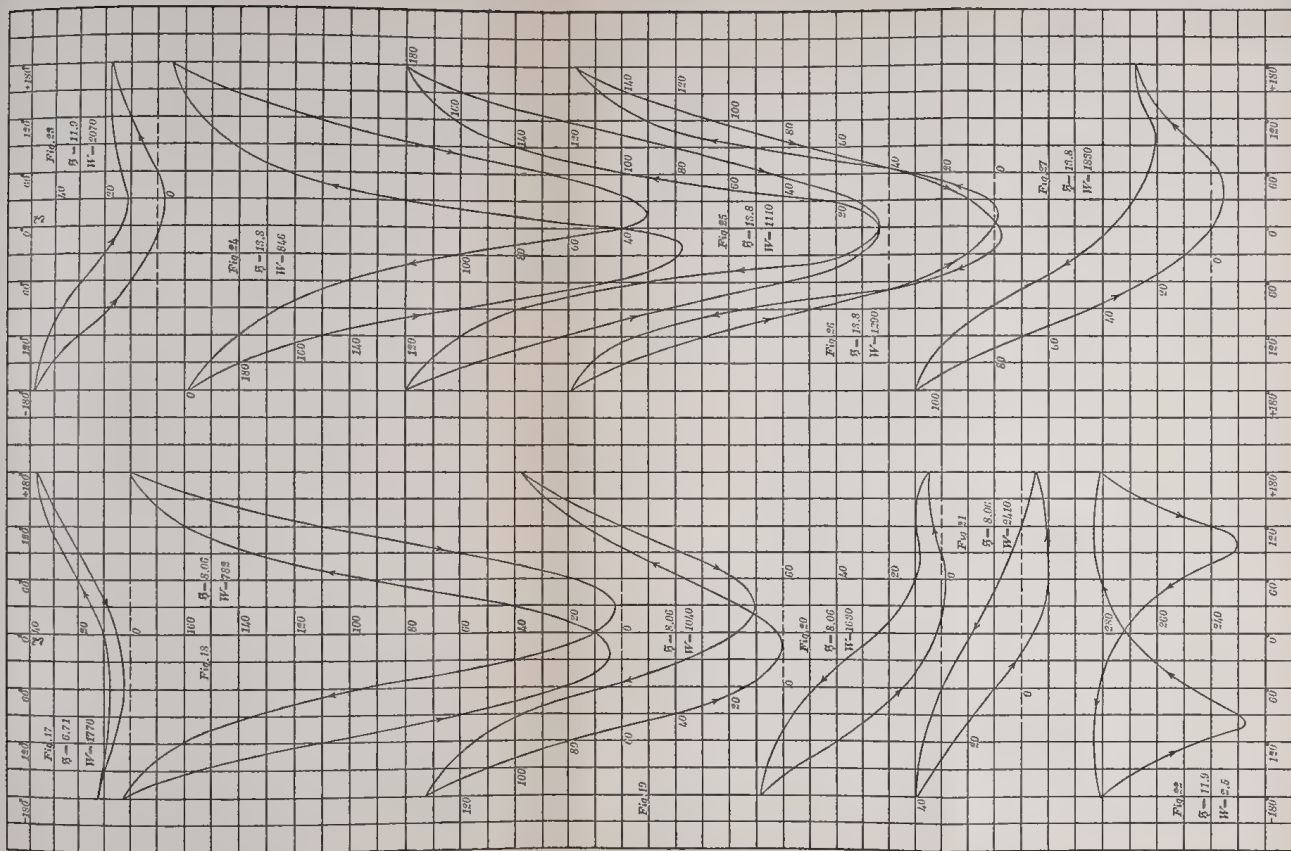


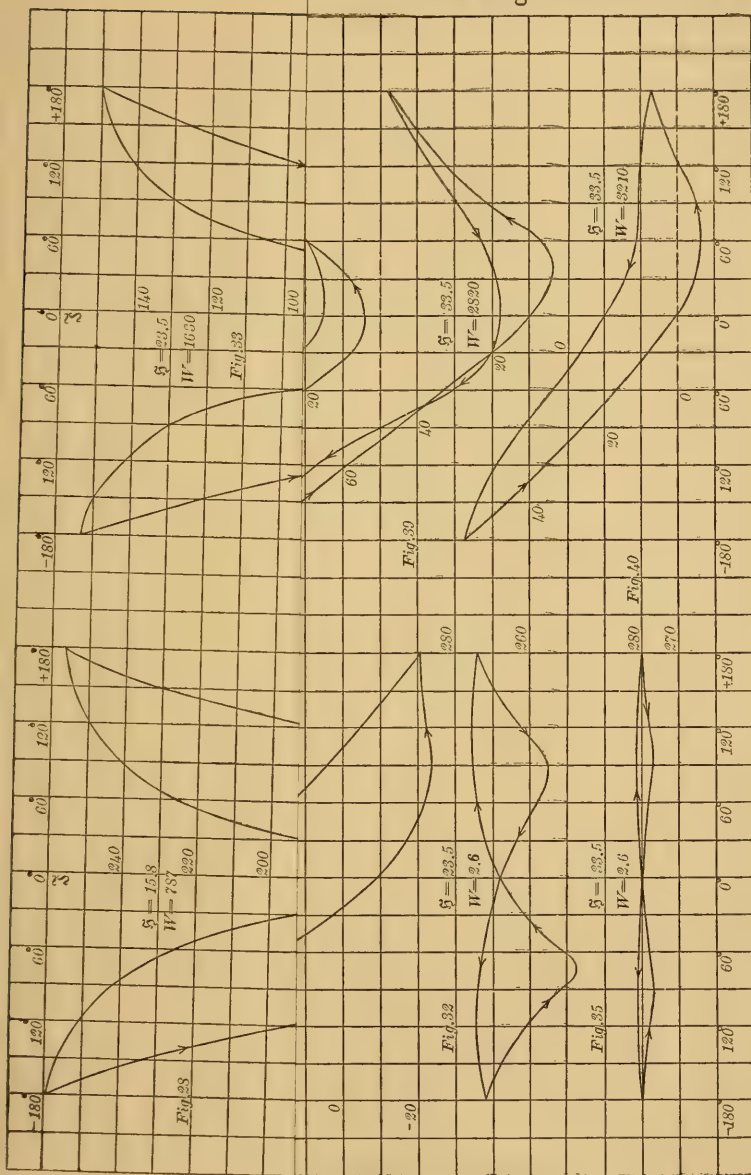


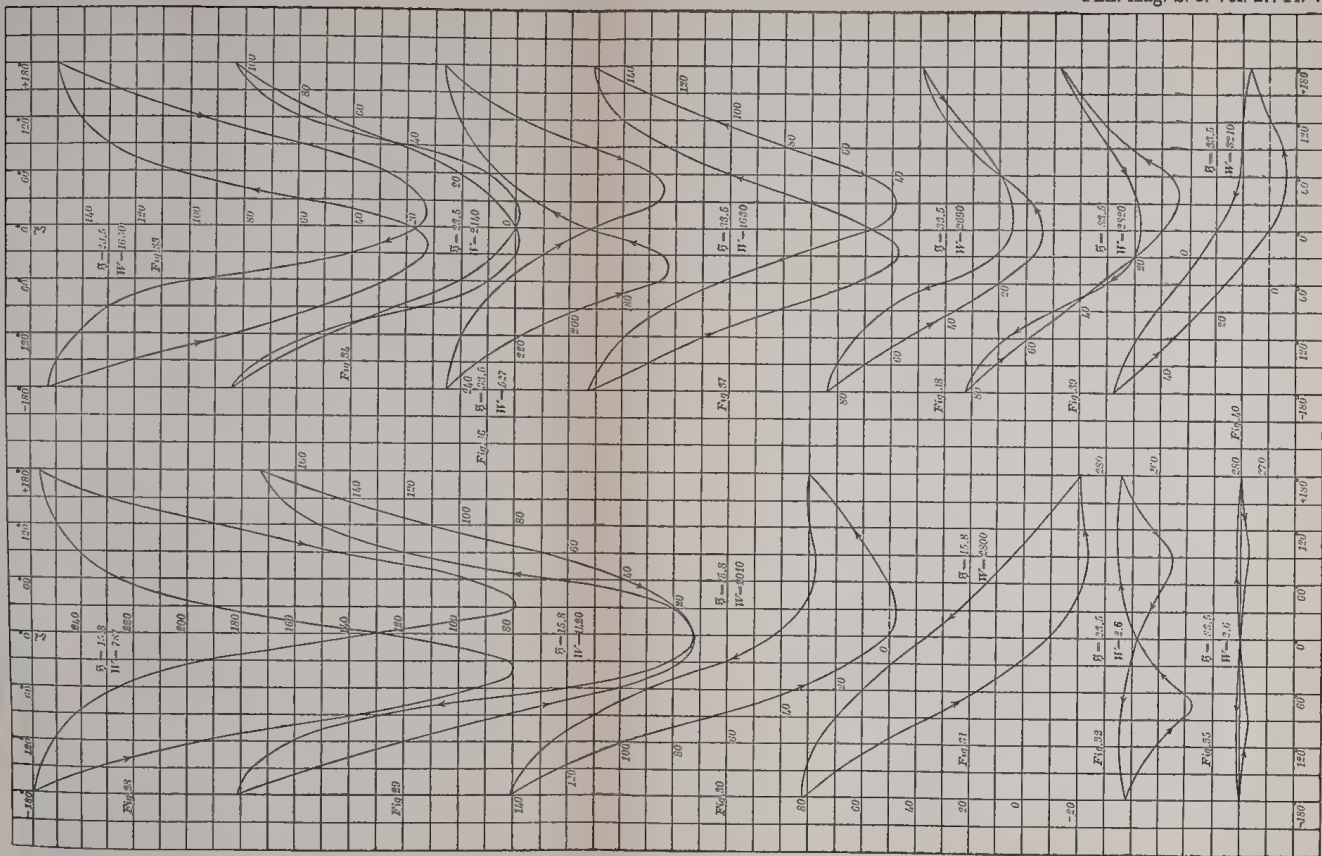


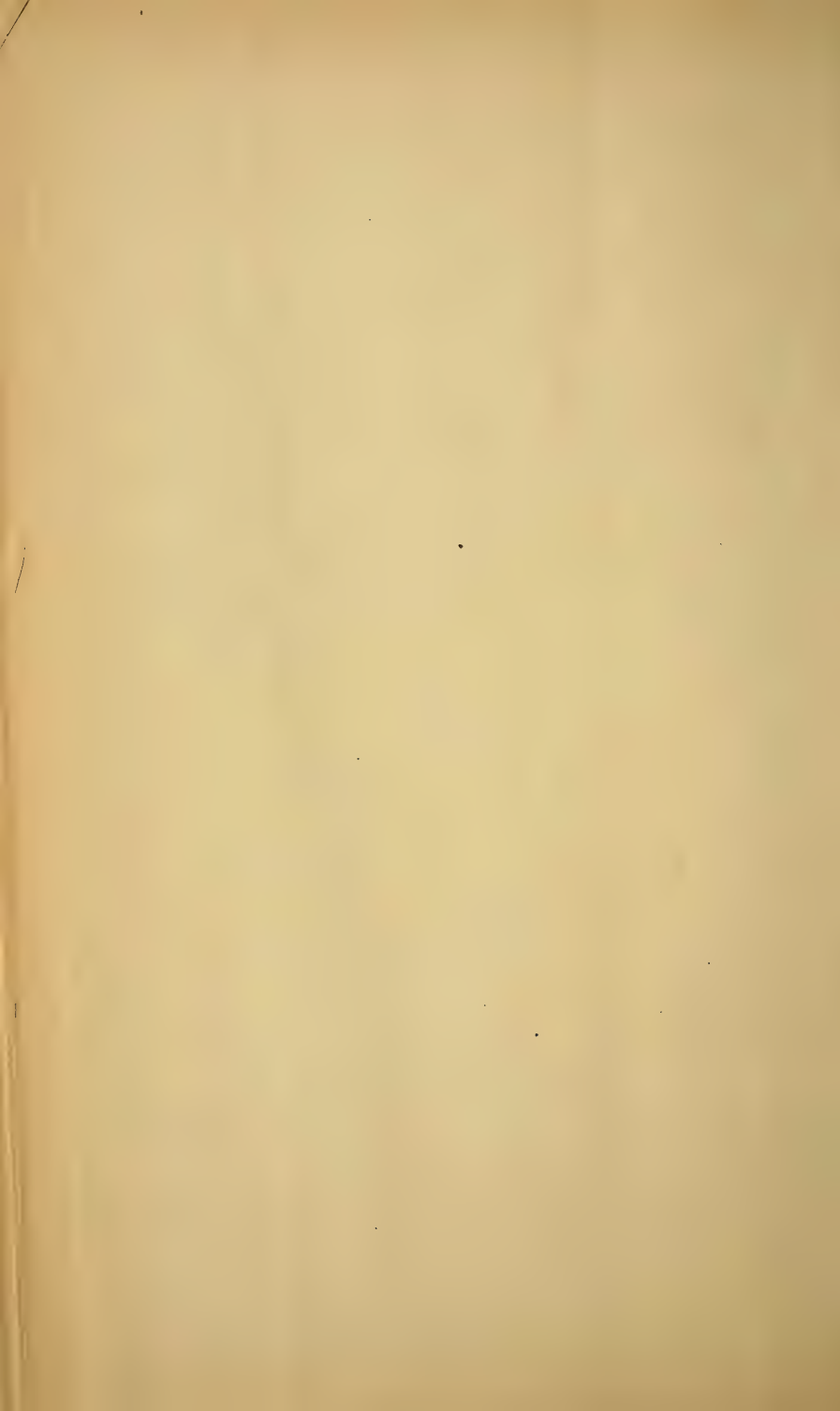


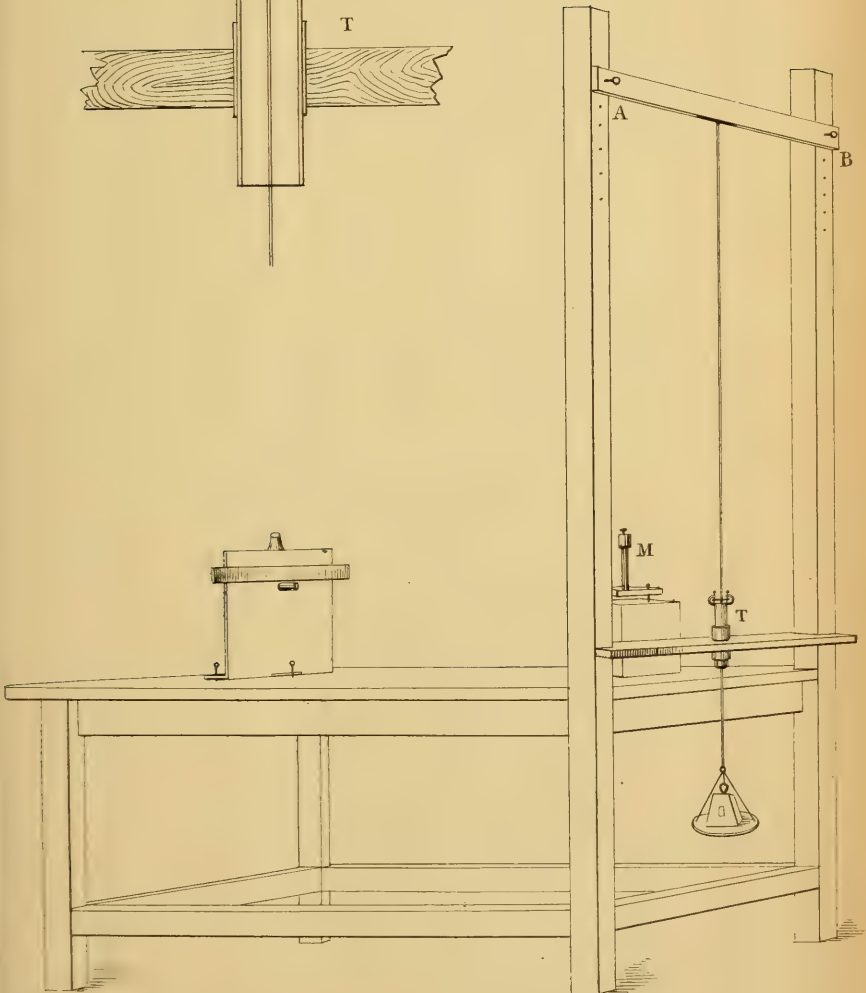
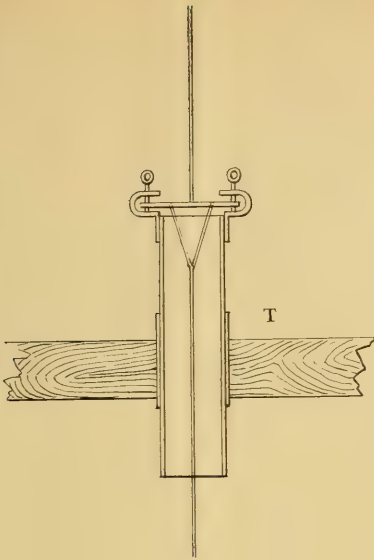












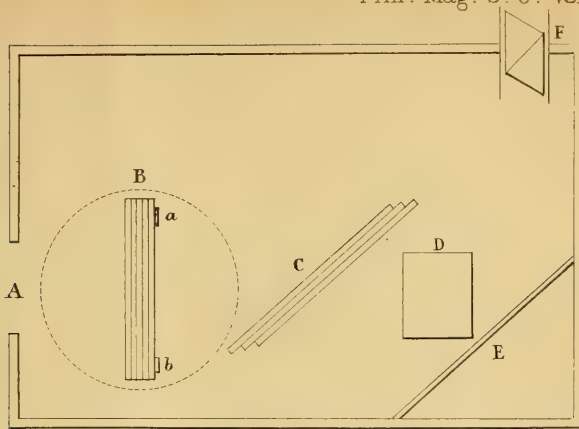


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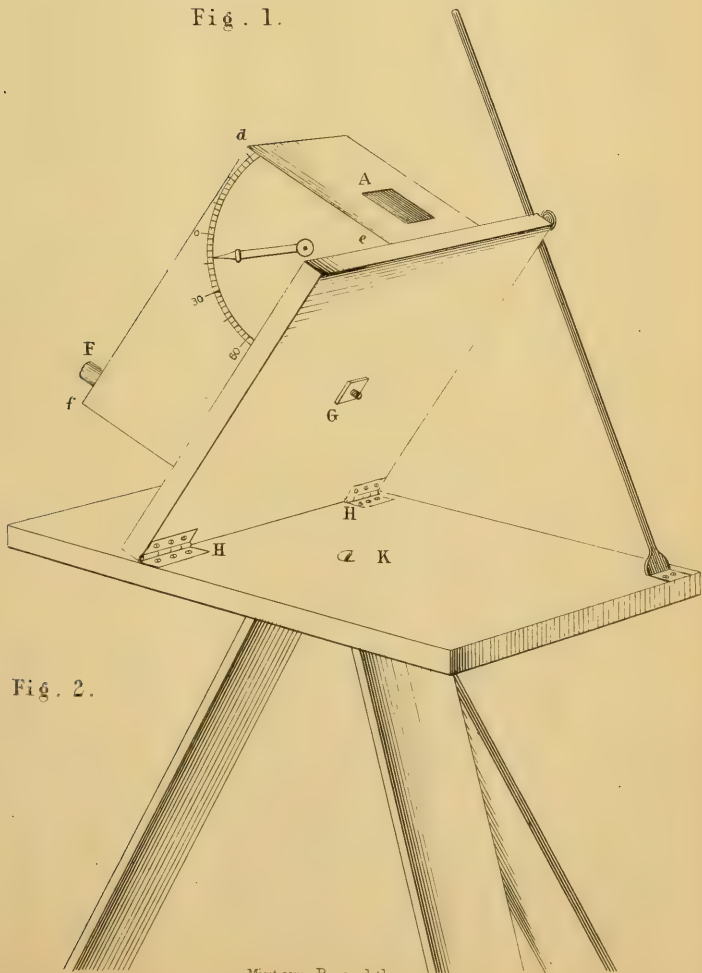


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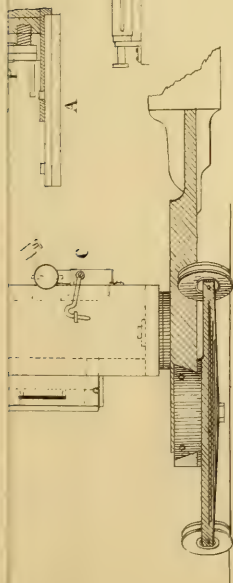


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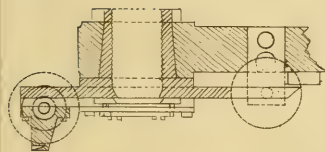
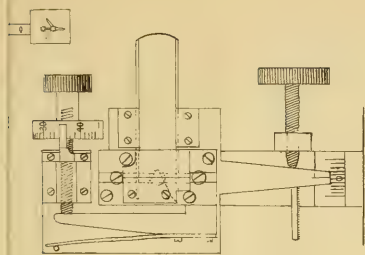
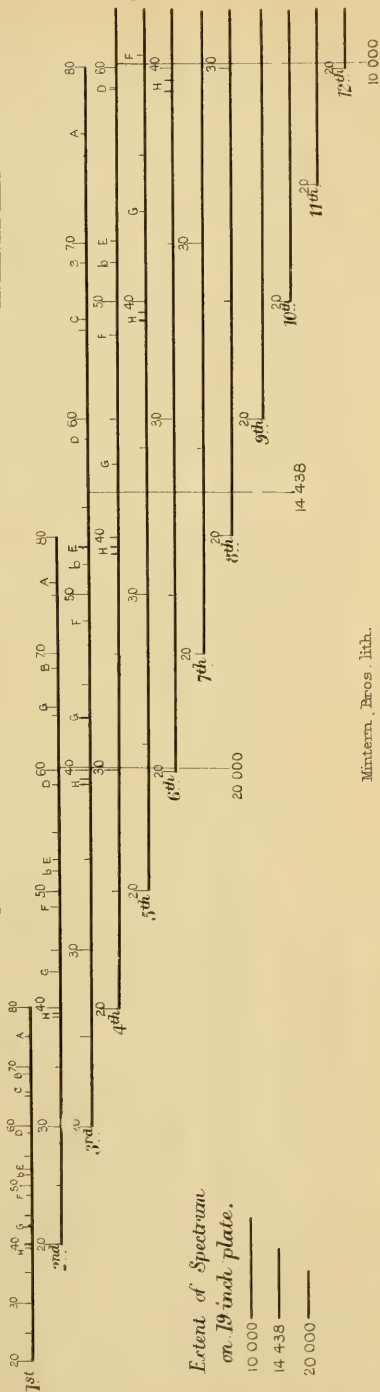


Fig. XVII.



OVERLAPPING SPECTRA.



Extent of Spectrum
on 19 inch plate.

10 000
14 438
20 000

Mintern, Bros. lith.

Fig. I.

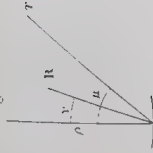


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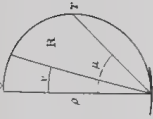


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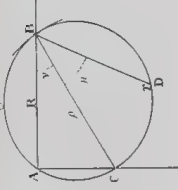


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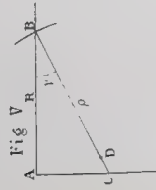
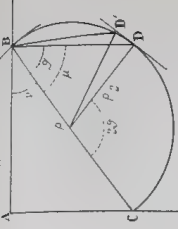


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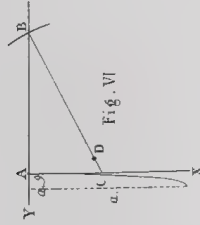


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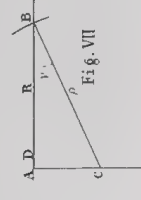


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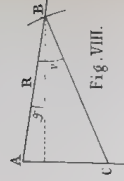


Fig. VIII.

Fig. IX.



Fig. X.

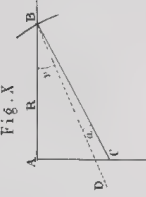


Fig. XI.



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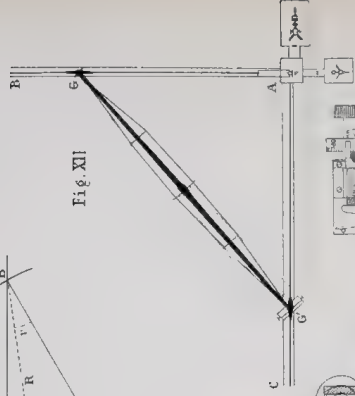


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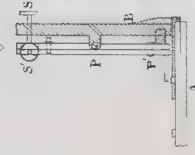


Fig. XIII.



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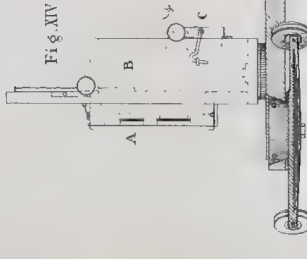
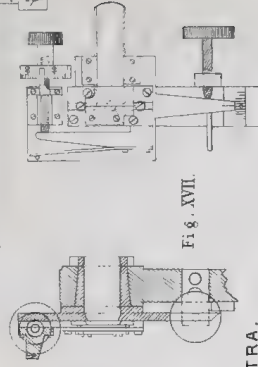


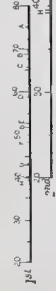
Fig. XV.



Fig. XVII.



OVERLAPPING SPECTRA.



Extent of Spectra
on 18 inch plate.

10 000
14 438
40 000

10 000
14 438
40 000

10 000
14 438
40 000

10 000
14 438
40 000

10 000
14 438
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10 000
14 438
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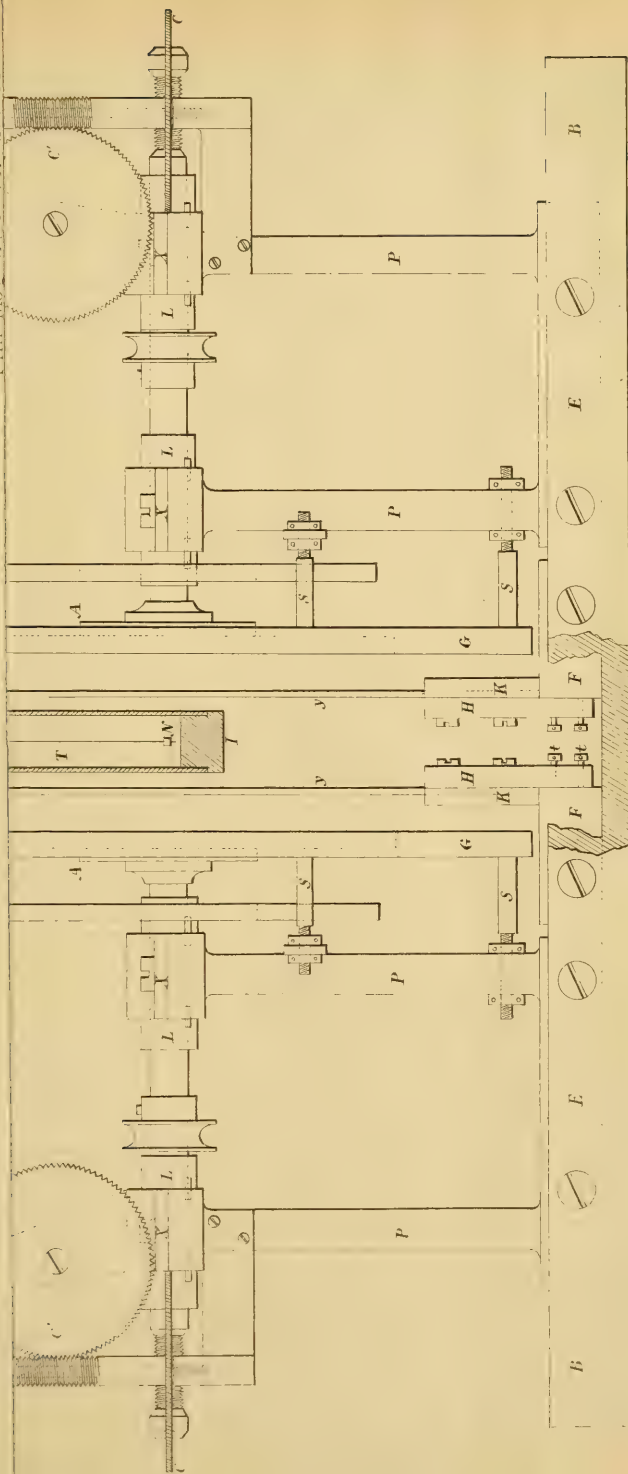
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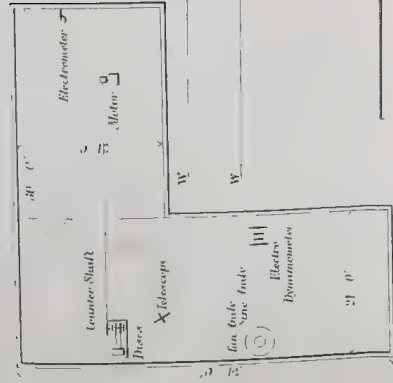


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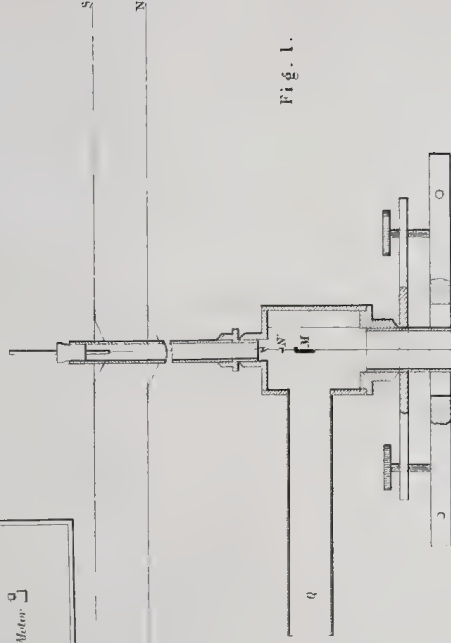
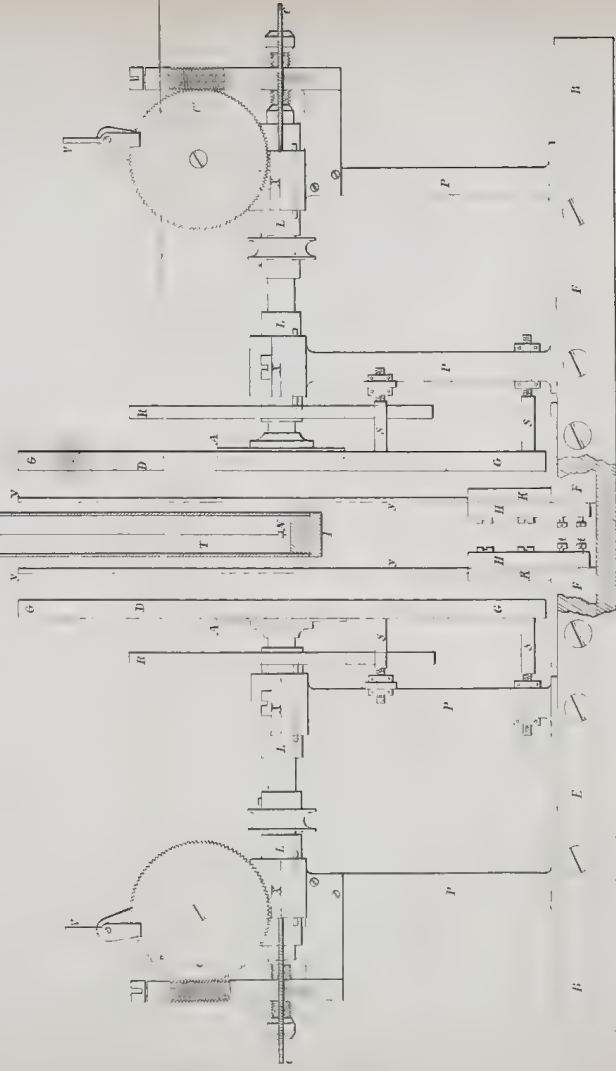


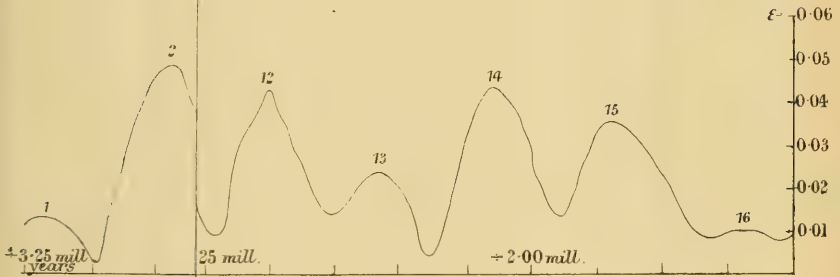
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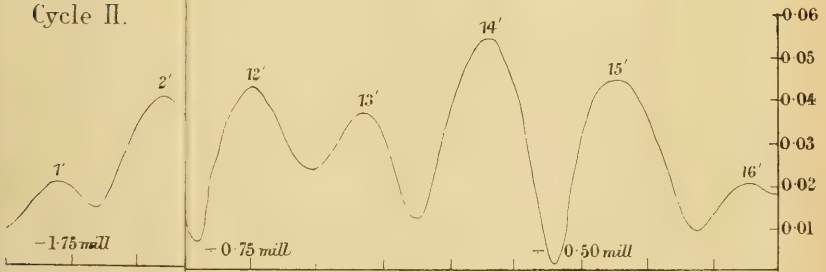
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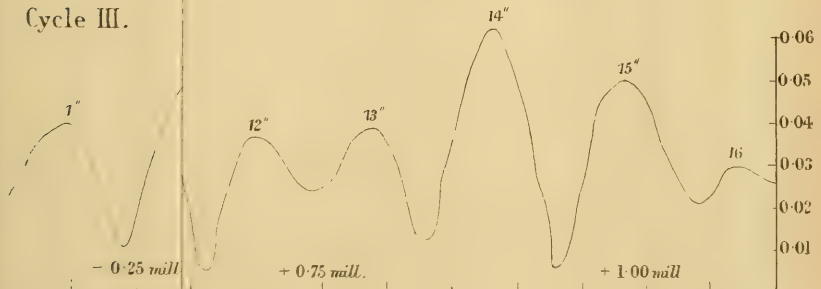
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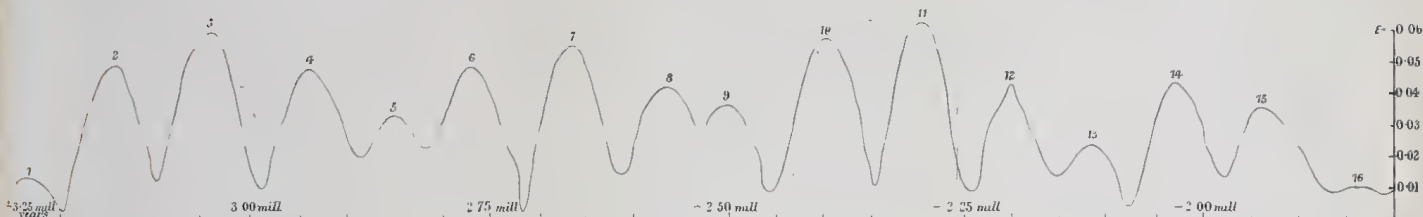
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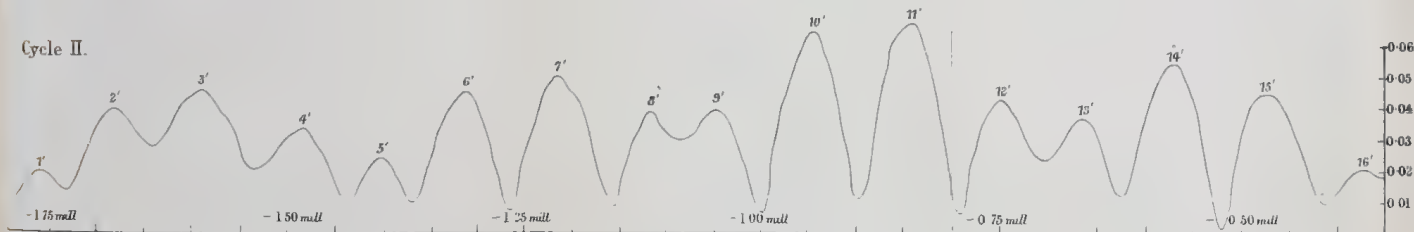
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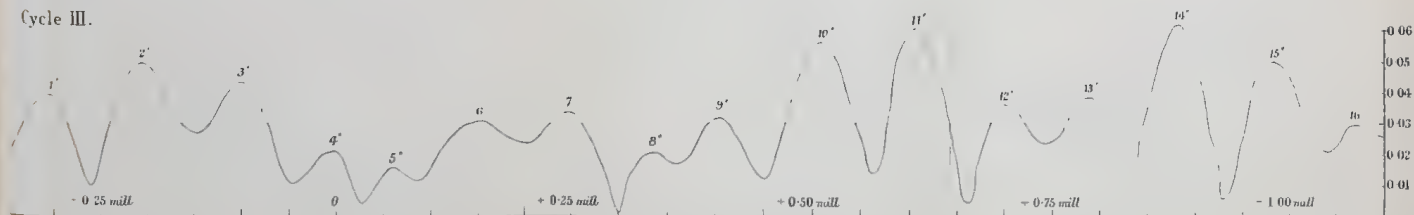
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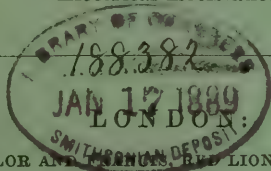
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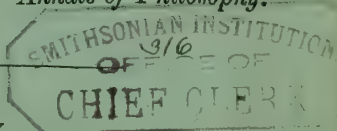
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By BENJAMIN WILLIAMSON, M.A., F.R.S.,

AND

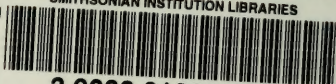
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